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Facts and Values

Although the study of values traditionally has belonged to ethics, the behavioral sciences recently have entered this realm. As they penetrate deeper, however, they find themselves facing the same obstacles that halted their predecessors.

The first foray into this territory is easy. Despite their special quality, values can come under scientific study. A judgment of value, as opposed to a statement of fact, is neither true nor false. To express its special quality requires the notion of *should*. For example, in passing judgment on the proper use of administrative finances, one might say, "You *should* not pocket departmental funds." To bring a judgment of value under scientific scrutiny, an investigator need only ask a question of fact about it. For example, he might ask, "What does the administration in such and such a university think of the pocketing of departmental funds?" Additional questions are likely to concern how such values are learned and what functions they serve.

The use to which a given body of knowledge is put depends in part upon the values held by the person or persons using that knowledge. For example, to launch a satellite requires some knowledge about the laws of physics, but the decision to use that knowledge is not itself a matter of physics. The decision rests on a complex system of values which, although difficult to express, culminates in the judgment that available funds should be spent to further the IGY program rather than, say, to reduce the national debt.

The terrain now grows more difficult. Instead of the control of natural forces, consider the control of men through the manipulation of the values that affect their behavior. That is to say, suppose that a behavioral technologist seeks to apply the knowledge gained by a behavioral theorist. Since this use of knowledge must also be based on values, the following question arises: Are these values in turn to be the subject of an investigation, with the information so gained in turn to be applied?

There appear to be two answers to this question, neither of which is satisfactory. To answer the question in the negative is to deny the claim with which the behavioral scientists entered the field, namely, that science can deal with values. To answer in the affirmative is merely to pose the question again, but once removed. Consequently, science in pursuit of values is much like a donkey in pursuit of a bunch of carrots that has been suspended in front of its nose from a twig fastened to its harness.

At a recent meeting of the American Psychological Association in Chicago, two distinguished students of the behavioral sciences debated the use of science in the control of human behavior. Although the protagonists did not settle the relationship between facts and values to everyone's satisfaction, let alone each other's, the discussion was sufficiently illuminating to call for its publication in this issue of *Science*.

If important questions of ethics are still unanswered, it may be something of a comfort to remember that the behavioral sciences are not yet in a position to produce results on order, whether for universal well-being, total slavery, or a bit of both.—J. T.

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Some Issues Concerning the Control of Human Behavior

A Symposium

Carl R. Rogers and B. F. Skinner

I [Skinner]

Science is steadily increasing our power to influence, change, mold—in a word, control—human behavior. It has extended our "understanding" (whatever that may be) so that we deal more successfully with people in nonscientific ways, but it has also identified conditions or variables which can be used to predict and control behavior in a new, and increasingly rigorous, technology. The broad disciplines of government and economics offer examples of this, but there is special cogency in those contributions of anthropology, sociology, and psychology which deal with individual behavior. Carl Rogers has listed some of the achievements to date in a recent paper (*1*). Those of his examples which show or imply the control of the single organism are primarily due, as we should expect, to psychology. It is the experimental study of behavior which carries us beyond awkward or inaccessible "principles," "factors," and so on, to variables which can be directly manipulated.

It is also, and for more or less the same reasons, the conception of human behavior emerging from an experimental analysis which most directly challenges traditional views. Psychologists themselves often do not seem to be aware of how far they have moved in this direction. But the change is not passing unnoticed by others. Until only recently it was customary to deny the possibility of a rigorous science of human behavior by arguing, either that a lawful science was impossible because man was a free agent, or that merely statistical predictions would always leave room for personal freedom. But those who used to take this line have become most vociferous in expressing their alarm at the way these obstacles are being surmounted.

Now, the control of human behavior has always been unpopular. Any undisguised effort to control usually arouses emotional reactions. We hesitate to admit, even to ourselves, that we are engaged in control, and we may refuse to control, even when this would be helpful, for fear of criticism. Those who have explicitly avowed an interest in control have been roughly treated by history. Machiavelli is the great prototype. As Macaulay said of him, "Out of his surname they coined an epithet for a knave and out of his Christian name a synonym for the devil." There were obvious reasons. The control that Machiavelli analyzed and recommended, like most political control, used techniques that were aversive to the controllee. The threats and punishments of the bully, like those of the government operating on the same plan, are not designed—whatever their success—to endear themselves to those who are controlled. Even when the techniques themselves are not aversive, control is usually exercised for the selfish purposes of the controller and, hence, has indirectly punishing effects upon others.

Man's natural inclination to revolt against selfish control has been exploited to good purpose in what we call the philosophy and literature of democracy. The doctrine of the rights of man has been effective in arousing individuals to concerted action against governmental and religious tyranny. The literature which has had this effect has greatly extended the number of terms in our language which express reactions to the control of men. But the ubiquity and ease of expression of this attitude spells trouble for any science which may give birth to a powerful technology of behavior. Intelligent men and women, dominated by the humanistic philosophy of the past two centuries, cannot view with equanimity what Andrew Hacker has called "the specter of predictable man" (*2*). Even the statistical or actuarial prediction of human events, such as the number of fatalities to be expected on a holiday weekend, strikes many people as uncanny and evil, while the prediction and control of individual behavior is regarded as little less than the work of the devil. I am not so much concerned here with the political or economic consequences for psychology, although research following certain channels may well suffer harmful effects. We ourselves, as intelligent men and women, and as exponents of Western thought, share these attitudes. They have already interfered with the free exercise of a scientific analysis, and their influence threatens to assume more serious proportions.

Three broad areas of human behavior supply good examples. The first of these —*personal control*—may be taken to include person-to-person relationships in the family, among friends, in social and work groups, and in counseling and psychotherapy. Other fields are *education* and *government*. A few examples from each will show how nonscientific preconceptions are affecting our current thinking about human behavior.

Personal Control

People living together in groups come to control one another with a technique which is not inappropriately called "ethical." When an individual behaves in a fashion acceptable to the group, he receives admiration, approval, affection, and many other reinforcements which increase the likelihood that he will continue to behave in that fashion. When his behavior is not acceptable, he is criticized, censured, blamed, or otherwise

Dr. Rogers is professor of psychology at the University of Chicago, and Dr. Skinner is professor of psychology at Harvard University. This article is based on material presented by the authors in a symposium held at the annual meeting of the American Psychological Association on 4 Sept. 1956 in Chicago, Ill.

punished. In the first case the group calls him "good"; in the second, "bad." This practice is so thoroughly ingrained in our culture that we often fail to see that it is a technique of control. Yet we are almost always engaged in such control, even though the reinforcements and punishments are often subtle.

The practice of admiration is an important part of a culture, because behavior which is otherwise inclined to be weak can be set up and maintained with its help. The individual is especially likely to be praised, admired, or loved when he acts for the group in the face of great danger, for example, or sacrifices himself or his possessions, or submits to prolonged hardship, or suffers martyrdom. These actions are not admirable in any absolute sense, but they require admiration if they are to be strong. Similarly, we admire people who behave in original or exceptional ways, not because such behavior is itself admirable, but because we do not know how to encourage original or exceptional behavior in any other way. The group acclaims independent, unaided behavior in part because it is easier to reinforce than to help.

As long as this technique of control is misunderstood, we cannot judge correctly an environment in which there is less need for heroism, hardship, or independent action. We are likely to argue that such an environment is itself less admirable or produces less admirable people. In the old days, for example, young scholars often lived in undesirable quarters, ate unappetizing or inadequate food, performed unprofitable tasks for a living or to pay for necessary books and materials or publication. Older scholars and other members of the group offered compensating reinforcement in the form of approval and admiration for these sacrifices. When the modern graduate student receives a generous scholarship, enjoys good living conditions, and has his research and publication subsidized, the grounds for evaluation seem to be pulled from under us. Such a student no longer *needs* admiration to carry him over a series of obstacles (no matter how much he may need it for other reasons), and, in missing certain familiar objects of admiration, we are likely to conclude that such *conditions* are less admirable. Obstacles to scholarly work may serve as a useful measure of motivation—and we may go wrong unless some substitute is found—but we can scarcely defend a deliberate harassment of the student for this purpose. The productivity of any set of conditions can be evaluated only when we have freed ourselves of the attitudes which have been generated in us as members of an ethical group.

A similar difficulty arises from our use of punishment in the form of censure or blame. The concept of responsibility and the related concepts of foreknowledge and choice are used to justify techniques of control using punishment. Was So-and-So aware of the probable consequences of his action, and was the action deliberate? If so, we are justified in punishing him. But what does this mean? It appears to be a question concerning the efficacy of the contingent relations between behavior and punishing consequences. We punish behavior because it is objectionable to us or the group, but in a minor refinement of rather recent origin we have come to withhold punishment when it cannot be expected to have any effect. If the objectionable consequences of an act were accidental and not likely to occur again, there is no point in punishing. We say that the individual was not "aware of the consequences of his action" or that the consequences were not "intentional." If the action could not have been avoided—if the individual "had no choice"—punishment is also withheld, as it is if the individual is incapable of being changed by punishment because he is of "unsound mind." In all these cases—different as they are—the individual is held "not responsible" and goes unpunished.

Just as we say that it is "not fair" to punish a man for something he could not help doing, so we call it "unfair" when one is rewarded beyond his due or for something he could not help doing. In other words, we also object to wasting *reinforcers* where they are not needed or will do no good. We make the same point with the words *just* and *right*. Thus we have no right to punish the irresponsible, and a man has no right to reinforcers he does not earn or deserve. But concepts of choice, responsibility, justice, and so on, provide a most inadequate analysis of efficient reinforcing and punishing contingencies because they carry a heavy semantic cargo of a quite different sort, which obscures any attempt to clarify controlling practices or to improve techniques. In particular, they fail to prepare us for techniques based on other than aversive techniques of control. Most people would object to forcing prisoners to serve as subjects of dangerous medical experiments, but few object when they are induced to serve by the offer of return privileges—even when the reinforcing effect of these privileges has been created by forcible deprivation. In the traditional scheme the right to refuse guarantees the individual against coercion or an unfair bargain. But to what extent *can* a prisoner refuse under such circumstances?

We need not go so far afield to make the point. We can observe our own attitude toward personal freedom in the way we resent any interference with what we want to do. Suppose we want to buy a car of a particular sort. Then we may object, for example, if our wife urges us to buy a less expensive model and to put the difference into a new refrigerator. Or we may resent it if our neighbor questions our need for such a car or our ability to pay for it. We would certainly resent it if it were illegal to buy such a car (remember Prohibition); and if we find we cannot actually afford it, we may resent governmental control of the price through tariffs and taxes. We resent it if we discover that we cannot get the car because the manufacturer is holding the model in deliberately short supply in order to push a model we do not want. In all this we assert our democratic right to buy the car of our choice. We are well prepared to do so and to resent any restriction on our freedom.

But why do we not ask *why* it is the car of our choice and resent the forces which made it so? Perhaps our favorite toy as a child was a car, of a very different model, but nevertheless bearing the name of the car we now want. Perhaps our favorite TV program is sponsored by the manufacturer of that car. Perhaps we have seen pictures of many beautiful or prestigeful persons driving it—in pleasant or glamorous places. Perhaps the car has been designed with respect to our motivational patterns: the device on the hood is a phallic symbol; or the horsepower has been stepped up to please our competitive spirit in enabling us to pass other cars swiftly (or, as the advertisements say, "safely"). The concept of freedom that has emerged as part of the cultural practice of our group makes little or no provision for recognizing or dealing with these kinds of control. Concepts like "responsibility" and "rights" are scarcely applicable. We are prepared to deal with coercive measures, but we have no traditional recourse with respect to other measures which in the long run (and especially with the help of science) may be much more powerful and dangerous.

Education

The techniques of education were once frankly aversive. The teacher was usually older and stronger than his pupils and was able to "make them learn." This meant that they were not actually taught but were surrounded by a threatening world from which they could escape only by learning. Usually they were left to their own resources in discovering how to do so. Claude Coleman has published a grimly amusing reminder of these older practices (*3*). He tells of a schoolteacher who published a careful account of his services during 51 years of teaching, during which he administered: ". . . 911,527 blows with a cane; 124,010 with a rod; 20,989 with a ruler; 136,715 with the hand; 10,295 over the mouth; 7,905 boxes on the ear; [and] 1,115,800 slaps on the head. . . ."

Progressive education was a humanitarian effort to substitute positive reinforcement for such aversive measures, but in the search for useful human values in the classroom it has never fully replaced the variables it abandoned. Viewed as a branch of behavioral technology, education remains relatively inefficient. We supplement it, and rationalize it, by admiring the pupil who learns *for himself*; and we often attribute the learning process, or knowledge itself, to something *inside* the individual. We admire behavior which seems to have inner sources. Thus we admire one who *recites* a poem more than one who simply *reads* it. We admire one who *knows* the answer more than one who *knows where to look it up*. We admire the *writer* rather than the *reader*. We admire the arithmetician who can do a problem in his head rather than with a slide rule or calculating machine, or in "original" ways rather than by a strict application of rules. In general we feel that any aid or "crutch"—except those aids to which we are now thoroughly accustomed—reduces the credit due. In Plato's *Phaedus*, Thamus, the king, attacks the invention of the alphabet on similar grounds! He is afraid "it will produce forgetfulness in the minds of those who learn to use it, because they will not practice their memories. . . ." In other words, he holds it more admirable to remember than to use a memorandum. He also objects that pupils "will read many things without instruction. . . [and] will therefore seem to know many things when they are for the most part ignorant." In the same vein we are today sometimes contemptuous of book learning, but, as educators, we can scarcely afford to adopt this view without reservation.

By admiring the student for knowledge and blaming him for ignorance, we escape some of the responsibility of teaching him. We resist any analysis of the educational process which threatens the notion of inner wisdom or questions the contention that the fault of ignorance lies with the student. More powerful techniques which bring about the same changes in behavior by manipulating *external* variables are decried as brainwashing or thought control. We are quite unprepared to judge *effective* educational measures. As long as only a few pupils learn much of what is taught, we do not worry about uniformity or regimentation. We do not fear the feeble technique; but we should view with dismay a system under which every student learned everything listed in a syllabus—although such a condition is far from unthinkable. Similarly, we do not fear a system which is so defective that the student must *work* for an education; but we are loath to give credit for anything learned without effort—although this could well be taken as an ideal result—and we flatly refuse to give credit if the student already knows what a school teaches.

A world in which people are wise and good without trying, without "having to be," without "choosing to be," could conceivably be a far better world for everyone. In such a world we should not have to "give anyone credit"—we should not need to admire anyone—for being wise and good. From our present point of view we cannot believe that such a world would be admirable. We do not even permit ourselves to imagine what it would be like.

Government

Government has always been the special field of aversive control. The state is frequently defined in terms of the power to punish, and jurisprudence leans heavily upon the associated notion of personal responsibility. Yet it is becoming increasingly difficult to reconcile current practice and theory with these earlier views. In criminology, for example, there is a strong tendency to drop the notion of responsibility in favor of some such alternative as capacity or controllability. But no matter how strongly the facts, or even practical expedience, support such a change, it is difficult to make the change in a legal system designed on a different plan. When governments resort to other techniques (for example, positive reinforcement), the concept of responsibility is no longer relevant and the theory of government is no longer applicable.

The conflict is illustrated by two decisions of the Supreme Court in the 1930's which dealt with, and disagreed on, the definition of control or coercion (*4*, p. 233). The Agricultural Adjustment Act proposed that the Secretary of Agriculture make "rental or benefit payments" to those farmers who agreed to reduce production. The government agreed that the Act would be unconstitutional if the farmer had been *compelled* to reduce production but was not, since he was merely *invited* to do so. Justice Roberts (*4*) expressed the contrary majority view of the court that "The power to confer or withhold unlimited benefits is the power to coerce or destroy." This recognition of positive reinforcement was withdrawn a few years later in another case in which Justice Cardozo (*4*, p. 244) wrote "To hold that motive or temptation is equivalent to coercion is to plunge the law in endless difficulties." We may agree with him, without implying that the proposition is therefore wrong. Sooner or later the law must be prepared to deal with all possible techniques of governmental control.

The uneasiness with which we view government (in the broadest possible sense) when it does not use punishment is shown by the reception of my utopian novel, *Walden Two* (*4a*). This was essentially a proposal to apply a behavioral technology to the construction of a workable, effective, and productive pattern of government. It was greeted with wrathful violence. *Life* magazine called it "a travesty on the good life," and "a menace . . . a triumph of mortmain or the dead hand not envisaged since the days of Sparta . . . a slur upon a name, a corruption of an impulse." Joseph Wood Krutch devoted a substantial part of his book, *The Measure of Man* (*5*), to attacking my views and those of the protagonist, Frazier, in the same vein, and Morris Viteles has recently criticized the book is a similar manner in *Science* (*6*). Perhaps the reaction is best expressed in a quotation from *The Quest for Utopia* by Negley and Patrick (*7*):

"Halfway through this contemporary utopia, the reader may feel sure, as we did, that this is a beautifully ironic satire on what has been called 'behavioral engineering.' The longer one stays in this better world of the psychologist, however, the plainer it becomes that the inspiration is not satiric, but messianic. This is indeed the behaviorally engineered society, and while it was to be expected that sooner or later the principle of psychological conditioning would be made the basis of a serious construction of utopia—Brown anticipated it in *Limanora*—yet not even the effective satire of Huxley is adequate preparation for the shocking horror of the idea when positively presented. Of all the dictatorships espoused by utopists, this is the most profound, and incipient dictators might well find in this utopia a guidebook of political practice."

One would scarcely guess that the authors are talking about a world in which there is food, clothing, and shelter for all, where everyone chooses his own work and works on the average only 4 hours a day, where music and the arts flourish, where personal relationships develop under the most favorable circumstances, where education prepares every child for the social and intellectual life which lies before him, where—in short—people are truly happy, secure, productive, creative, and forward-looking. What is wrong with it? Only one thing: someone "planned it that way." If these critics had come upon a society in some remote corner of the world which boasted similar advantages, they would undoubtedly have hailed it as providing a pattern we all might well follow—provided that it was clearly the result of a natural process of cultural evolution. Any evidence that intelligence had been used in arriving at this version of the good life would, in their eyes, be a serious flaw. No matter if the planner of *Walden Two* diverts none of the proceeds of the community

to his own use, no matter if he has no current control or is, indeed, unknown to most of the other members of the community (he planned that, too), somewhere back of it all he occupies the position of prime mover. And this, to the child of the democratic tradition, spoils it all.

The dangers inherent in the control of human behavior are very real. The possibility of the misuse of scientific knowledge must always be faced. We cannot escape by denying the power of a science of behavior or arresting its development. It is no help to cling to familiar philosophies of human behavior simply because they are more reassuring. As I have pointed out elsewhere (*8*), the new techniques emerging from a science of behavior must be subject to the explicit countercontrol which has already been applied to earlier and cruder forms. Brute force and deception, for example, are now fairly generally suppressed by ethical practices and by explicit governmental and religious agencies. A similar countercontrol of scientific knowledge in the interests of the group is a feasible and promising possibility. Although we cannot say how devious the course of its evolution may be, a cultural pattern of control and countercontrol will presumably emerge which will be most widely supported because it is most widely reinforcing.

If we cannot foresee all the details of this (as we obviously cannot), it is important to remember that this is true of the critics of science as well. The dire consequences of new techniques of control, the hidden menace in original cultural designs—these need some proof. It is only another example of my present point that the need for proof is so often overlooked. Man has got himself into some pretty fixes, and it is easy to believe that he will do so again. But there is a more optimistic possibility. The slow growth of the methods of science, now for the first time being applied to human affairs, *may* mean a new and exciting phase of human life to which historical analogies will not apply and in which earlier political slogans will not be appropriate. If we are to use the knowledge that a science of behavior is now making available with any hope of success, we must look at human nature as it is brought into focus through the methods of science rather than as it has been presented to us in a series of historical accidents.

If the advent of a powerful science of behavior causes trouble, it will not be because science itself is inimical to human welfare but because older conceptions have not yielded easily or gracefully. We expect resistance to new techniques of control from those who have heavy investments in the old, but we have no reason to help them preserve a series of principles that are not ends in themselves but rather outmoded means to an end. What is needed is a new conception of human behavior which is compatible with the implications of a scientific analysis. All men control and are controlled. The question of government in the broadest possible sense is not how freedom is to be preserved but what kinds of control are to be used and to what ends. Control must be analyzed and considered in its proper proportions. No one, I am sure, wishes to develop new master-slave relationships or bend the will of the people to despotic rulers in new ways. These are patterns of control appropriate to a world without science. They may well be the first to go when the experimental analysis of behavior comes into its own in the design of cultural practices.

II [Rogers]

There are, I believe, a number of matters in connection with this important topic on which the authors of this article, and probably a large majority of psychologists, are in agreement. These matters then are not issues as far as we are concerned, and I should like to mention them briefly in order to put them to one side.

Points of Agreement

I am sure we agree that men—as individuals and as societies—have always endeavored to understand, predict, influence, and control human behavior—their own behavior and that of others.

I believe we agree that the behavioral sciences are making and will continue to make increasingly rapid progress in the understanding of behavior, and that as a consequence the capacity to predict and to control behavior is developing with equal rapidity.

I believe we agree that to deny these advances, or to claim that man's behavior cannot be a field of science, is unrealistic. Even though this is not an issue for us, we should recognize that many intelligent men still hold strongly to the view that the actions of men are free in some sense such that scientific knowledge of man's behavior is impossible. Thus Reinhold Niebuhr, the noted theologian, heaps scorn on the concept of psychology as a science of man's behavior and even says, "In any event, no scientific investigation of past behavior can become the basis of predictions of future behavior" (*9*). So, while this is not an issue for psychologists, we should at least notice in passing that it is an issue for many people.

I believe we are in agreement that the tremendous potential power of a science which permits the prediction and control of behavior may be misused, and that the possibility of such misuse constitutes a serious threat.

Consequently Skinner and I are in agreement that the whole question of the scientific control of human behavior is a matter with which psychologists and the general public should concern themselves. As Robert Oppenheimer told the American Psychological Association last year (*10*) the problems that psychologists will pose for society by their growing ability to control behavior will be much more grave than the problems posed by the ability of physicists to control the reactions of matter. I am not sure whether psychologists generally recognize this. My impression is that by and large they hold a laissez-faire attitude. Obviously Skinner and I do not hold this laissez-faire view, or we would not have written this article.

Points at Issue

With these several points of basic and important agreement, are there then any issues that remain on which there are differences? I believe there are. They can be stated very briefly: Who will be controlled? Who will exercise control? What type of control will be exercised? Most important of all, toward what end or what purpose, or in the pursuit of what value, will control be exercised?

It is on questions of this sort that there exist ambiguities, misunderstandings, and probably deep differences. These differences exist among psychologists, among members of the general public in this country, and among various world cultures. Without any hope of achieving a final resolution of these questions, we can, I believe, put these issues in clearer form.

Some Meanings

To avoid ambiguity and faulty communication, I would like to clarify the meanings of some of the terms we are using.

Behavioral science is a term that might be defined from several angles but in the context of this discussion it refers primarily to knowledge that the existence of certain describable conditions in the human being and/or in his environment is followed by certain describable consequences in his actions.

Prediction means the prior identification of behaviors which then occur. Because it is important in some things I wish to say later, I would point out that one may predict a highly specific behavior, such as an eye blink, or one may predict a class of behaviors. One might correctly predict "avoidant behavior," for example, without being able to spec-

ify whether the individual will run away or simply close his eyes.

The word *control* is a very slippery one, which can be used with any one of several meanings. I would like to specify three that seem most important for our present purposes. *Control* may mean: (i) The setting of conditions by *B* for *A*, *A* having no voice in the matter, such that certain predictable behaviors then occur in *A*. I refer to this as external control. (ii) The setting of conditions by *B* for *A*, *A* giving some degree of consent to these conditions, such that certain predictable behaviors then occur in *A*. I refer to this as the influence of *B* on *A*. (iii) The setting of conditions by *A* such that certain predictable behaviors then occur in himself. I refer to this as internal control. It will be noted that Skinner lumps together the first two meanings, external control and influence, under the concept of control. I find this confusing.

Usual Concept of Control of Human Behavior

With the underbrush thus cleared away (I hope), let us review very briefly the various elements that are involved in the usual concept of the control of human behavior as mediated by the behavorial sciences. I am drawing here on the previous writings of Skinner, on his present statements, on the writings of others who have considered in either friendly or antagonistic fashion the meanings that would be involved in such control. I have not excluded the science fiction writers, as reported recently by Vandenburg (*11*), since they often show an awareness of the issues involved, even though the methods described are as yet fictional. These then are the elements that seem common to these different concepts of the application of science to human behavior.

1) There must first be some sort of decision about goals. Usually desirable goals are assumed, but sometimes, as in George Orwell's book *1984*, the goal that is selected is an aggrandizement of individual power with which most of us would disagree. In a recent paper Skinner suggests that one possible set of goals to be assigned to the behavioral technology is this: "Let men be happy, informed, skillful, well-behaved and productive" (*12*). In the first draft of his part of this article, which he was kind enough to show me, he did not mention such definite goals as these, but desired "improved" educational practices, "wiser" use of knowledge in government, and the like. In the final version of his article he avoids even these value-laden terms, and his implicit goal is the very general one that scientific control of behavior is desirable, because it would perhaps bring "a far better world for everyone."

Thus the first step in thinking about the control of human behavior is the choice of goals, whether specific or general. It is necessary to come to terms in some way with the issue, "For what purpose?"

2) A second element is that, whether the end selected is highly specific or is a very general one such as wanting "a better world," we proceed by the methods of science to discover the means to these ends. We continue through further experimentation and investigation to discover more effective means. The method of science is self-correcting in thus arriving at increasingly effective ways of achieving the purpose we have in mind.

3) The third aspect of such control is that as the conditions or methods are discovered by which to reach the goal, some person or some group establishes these conditions and uses these methods, having in one way or another obtained the power to do so.

4) The fourth element is the exposure of individuals to the prescribed conditions, and this leads, with a high degree of probability, to behavior which is in line with the goals desired. Individuals are now happy, if that has been the goal, or well-behaved, or submissive, or whatever it has been decided to make them.

5) The fifth element is that if the process I have described is put in motion then there is a continuing social organization which will continue to produce the types of behavior that have been valued.

Some Flaws

Are there any flaws in this way of viewing the control of human behavior? I believe there are. In fact the only element in this description with which I find myself in agreement is the second. It seems to me quite incontrovertibly true that the scientific method is an excellent way to discover the means by which to achieve our goals. Beyond that, I feel many sharp differences, which I will try to spell out.

I believe that in Skinner's presentation here and in his previous writings, there is a serious underestimation of the problem of power. To hope that the power which is being made available by the behavioral sciences will be exercised by the scientists, or by a benevolent group, seems to me a hope little supported by either recent or distant history. It seems far more likely that behavioral scientists, holding their present attitudes, will be in the position of the German rocket scientists specializing in guided missiles. First they worked devotedly for Hitler to destroy the U.S.S.R. and the United States. Now, depending on who captured them, they work devotedly for the U.S.S.R. in the interest of destroying the United States, or devotedly for the United States in the interest of destroying the U.S.S.R. If behavioral scientists are concerned solely with advancing their science, it seems most probable that they will serve the purposes of whatever individual or group has the power.

But the major flaw I see in this review of what is involved in the scientific control of human behavior is the denial, misunderstanding, or gross underestimation of the place of ends, goals or values in their relationship to science. This error (as it seems to me) has so many implications that I would like to devote some space to it.

Ends and Values in Relation to Science

In sharp contradiction to some views that have been advanced, I would like to propose a two-pronged thesis: (i) In any scientific endeavor—whether "pure" or applied science—there is a prior subjective choice of the purpose or value which that scientific work is perceived as serving. (ii) This subjective value choice which brings the scientific endeavor into being must always lie outside of that endeavor and can never become a part of the science involved in that endeavor.

Let me illustrate the first point from Skinner himself. It is clear that in his earlier writing (*12*) it is recognized that a prior value choice is necessary, and it is specified as the goal that men are to become happy, well-behaved, productive, and so on. I am pleased that Skinner has retreated from the goals he then chose, because to me they seem to be stultifying values. I can only feel that he was choosing these goals for others, not for himself. I would hate to see Skinner become "well-behaved," as that term would be defined for him by behavioral scientists. His recent article in the *American Psychologist* (*13*) shows that he certainly does not want to be "productive" as that value is defined by most psychologists. And the most awful fate I can imagine for him would be to have him constantly "happy." It is the fact that he is very unhappy about many things which makes me prize him.

In the first draft of his part of this article, he also included such prior value choices, saying for example, "We must decide how we are to use the knowledge which a science of human behavior is now making available." Now he has dropped all mention of such choices, and if I understand him correctly, he believes that science can proceed without them. He has suggested this view in another recent paper, stating that "We must continue to experiment in cultural design . . . testing the consequences as we go. Eventually the practices which make for the greatest biological and psychological strength of the group will presumably survive" (*8*, p. 549).

I would point out, however, that to choose to experiment is a value choice. Even to move in the direction of perfectly random experimention is a value choice. To test the consequences of an experiment is possible only if we have first made a subjective choice of a criterion value. And implicit in his statement is a valuing of biological and psychological strength. So even when trying to avoid such choice, it seems inescapable that a prior subjective value choice is necessary for any scientific endeavor, or for any application of scientific knowledge.

I wish to make it clear that I am not saying that values cannot be included as a subject of science. It is not true that science deals only with certain classes of "facts" and that these classes do not include values. It is a bit more complex than that, as a simple illustration or two may make clear.

If I value knowledge of the "three R's" as a goal of education, the methods of science can give me increasingly accurate information on how this goal may be achieved. If I value problem-solving ability as a goal of education, the scientific method can give me the same kind of help.

Now, if I wish to determine whether problem-solving ability is "better" than knowledge of the three R's, then scientific method can also study those two values but *only*—and this is very important—in terms of some other value which I have subjectively chosen. I may value college success. Then I can determine whether problem-solving ability or knowledge of the three R's is most closely associated with that value. I may value personal integration or vocational success or responsible citizenship. I can determine whether problem-solving ability or knowledge of the three R's is "better" for achieving any one of these values. But the value or purpose that gives meaning to a particular scientific endeavor must always lie outside of that endeavor.

Although our concern in this symposium is largely with applied science, what I have been saying seems equally true of so-called "pure" science. In pure science the usual prior subjective value choice is the discovery of truth. But this is a subjective choice, and science can never say whether it is the best choice, save in the light of some other value. Geneticists in the U.S.S.R., for example, had to make a subjective choice of whether it was better to pursue truth or to discover facts which upheld a governmental dogma. Which choice is "better"? We could make a scientific investigation of those alternatives but only in the light of some other subjectively chosen value. If, for example, we value the survival of a culture, then we could begin to investigate with the methods of science the question of whether pursuit of truth or support of governmental dogma is most closely associated with cultural survival.

My point then is that any endeavor in science, pure or applied, is carried on in the pursuit of a purpose or value that is subjectively chosen by persons. It is important that this choice be made explicit, since the particular value which is being sought can never be tested or evaluated, confirmed or denied, by the scientific endeavor to which it gives birth. The initial purpose or value always and necessarily lies outside the scope of the scientific effort which it sets in motion.

Among other things this means that if we choose some particular goal or series of goals for human beings and then set out on a large scale to control human behavior to the end of achieving those goals, we are locked in the rigidity of our initial choice, because such a scientific endeavor can never transcend itself to select new goals. Only subjective human persons can do that. Thus if we chose as our goal the state of happiness for human beings (a goal deservedly ridiculed by Aldous Huxley in *Brave New World*), and if we involved all of society in a successful scientific program by which people became happy, we would be locked in a colossal rigidity in which no one would be free to question this goal, because our scientific operations could not transcend themselves to question their guiding purposes. And without laboring this point, I would remark that colossal rigidity, whether in dinosaurs or dictatorships, has a very poor record of evolutionary survival.

If, however, a part of our scheme is to set free some "planners" who do not have to be happy, who are not controlled, and who are therefore free to choose other values, this has several meanings. It means that the purpose we have chosen as our goal is not a sufficient and a satisfying one for human beings but must be supplemented. It also means that if it is necessary to set up an elite group which is free, then this shows all too clearly that the great majority are only the slaves—no matter by what high-sounding name we call them—of those who select the goals.

Perhaps, however, the thought is that a continuing scientific endeavor will evolve its own goals; that the initial findings will alter the directions, and subsequent findings will alter them still further, and that science somehow develops its own purpose. Although he does not clearly say so, this appears to be the pattern Skinner has in mind. It is surely a reasonable description, but it overlooks one element in this continuing development, which is that subjective personal choice enters in at every point at which the direction changes. The findings of a science, the results of an experiment, do not and never can tell us what next scientific purpose to pursue. Even in the purest of science, the scientist must decide what the findings mean and must subjectively choose what next step will be most profitable in the pursuit of his purpose. And if we are speaking of the application of scientific knowledge, then it is distressingly clear that the increasing scientific knowledge of the structure of the atom carries with it no necessary choice as to the purpose to which this knowledge will be put. This is a subjective personal choice which must be made by many individuals.

Thus I return to the proposition with which I began this section of my remarks—and which I now repeat in different words. Science has its meaning as the objective pursuit of a purpose which has been subjectively chosen by a person or persons. This purpose or value can never be investigated by the particular scientific experiment or investigation to which it has given birth and meaning. Consequently, any discussion of the control of human beings by the behavioral sciences must first and most deeply concern itself with the subjectively chosen purposes which such an application of science is intended to implement.

Is the Situation Hopeless?

The thoughtful reader may recognize that, although my remarks up to this point have introduced some modifications in the conception of the processes by which human behavior will be controlled, these remarks may have made such control seem, if anything, even more inevitable. We might sum it up this way: Behavioral science is clearly moving forward; the increasing power for control which it gives will be held by someone or some group; such an individual or group will surely choose the values or goals to be achieved; and most of us will then be increasingly controlled by means so subtle that we will not even be aware of them as controls. Thus, whether a council of wise psychologists (if this is not a contradiction in terms), or a Stalin, or a Big Brother has the power, and whether the goal is happiness, or productivity, or resolution of the Oedipus complex, or submission, or love of Big Brother, we will inevitably find ourselves moving toward the chosen goal and probably thinking that we ourselves desire it. Thus, if this line of reasoning is correct, it appears that some form of *Walden Two* or of *1984* (and at a deep philosophic level they seem indistinguishable) is coming. The fact that it would surely arrive piecemeal, rather than all at once, does not greatly change the fundamental

issues. In any event, as Skinner has indicated in his writings, we would then look back upon the concepts of human freedom, the capacity for choice, the responsibility for choice, and the worth of the human individual as historical curiosities which once existed by cultural accident as values in a prescientific civilization.

I believe that any person observant of trends must regard something like the foregoing sequence as a real possibility. It is not simply a fantasy. Something of that sort may even be the most likely future. But is it an inevitable future? I want to devote the remainder of my remarks to an alternative possibility.

Alternative Set of Values

Suppose we start with a set of ends, values, purposes, quite different from the type of goals we have been considering. Suppose we do this quite openly, setting them forth as a possible value choice to be accepted or rejected. Suppose we select a set of values that focuses on fluid elements of process rather than static attributes. We might then value: man as a process of becoming, as a process of achieving worth and dignity through the development of his potentialities; the individual human being as a self-actualizing process, moving on to more challenging and enriching experiences; the process by which the individual creatively adapts to an ever-new and changing world; the process by which knowledge transcends itself, as, for example, the theory of relativity transcended Newtonian physics, itself to be transcended in some future day by a new perception.

If we select values such as these we turn to our science and technology of behavior with a very different set of questions. We will want to know such things as these: Can science aid in the discovery of new modes of richly rewarding living? more meaningful and satisfying modes of interpersonal relationships? Can science inform us on how the human race can become a more intelligent participant in its own evolution—its physical, psychological and social evolution? Can science inform us on ways of releasing the creative capacity of individuals, which seem so necessary if we are to survive in this fantastically expanding atomic age? Oppenheimer has pointed out (*14*) that knowledge, which used to double in millenia or centuries, now doubles in a generation or a decade. It appears that we must discover the utmost in release of creativity if we are to be able to adapt effectively. In short, can science discover the methods by which man can most readily become a continually developing and self-transcending process, in his behavior, his thinking, his knowledge? Can science predict and release an essentially "unpredictable" freedom?

It is one of the virtues of science as a method that it is as able to advance and implement goals and purposes of this sort as it is to serve static values, such as states of being well-informed, happy, obedient. Indeed we have some evidence of this.

Small Example

I will perhaps be forgiven if I document some of the possibilities along this line by turning to psychotherapy, the field I know best.

Psychotherapy, as Meerloo (*15*) and others have pointed out, can be one of the most subtle tools for the control of *A* by *B*. The therapist can subtly mold individuals in imitation of himself. He can cause an individual to become a submissive and conforming being. When certain therapeutic principles are used in extreme fashion, we call it brainwashing, an instance of the disintegration of the personality and a reformulation of the person along lines desired by the controlling individual. So the principles of therapy can be used as an effective means of external control of human personality and behavior. Can psychotherapy be anything else?

Here I find the developments going on in client-centered psychotherapy (*16*) an exciting hint of what a behavioral science can do in achieving the kinds of values I have stated. Quite aside from being a somewhat new orientation in psychotherapy, this development has important implications regarding the relation of a behavioral science to the control of human behavior. Let me describe our experience as it relates to the issues of this discussion.

In client-centered therapy, we are deeply engaged in the prediction and influencing of behavior, or even the control of behavior. As therapists, we institute certain attitudinal conditions, and the client has relatively little voice in the establishment of these conditions. We predict that if these conditions are instituted, certain behavioral consequences will ensue in the client. Up to this point this is largely external control, no different from what Skinner has described, and no different from what I have discussed in the preceding sections of this article. But here any similarity ceases.

The conditions we have chosen to establish predict such behavioral consequences as these: that the client will become self-directing, less rigid, more open to the evidence of his senses, better organized and integrated, more similar to the ideal which he has chosen for himself. In other words, we have established by external control conditions which we predict will be followed by internal control by the individual, in pursuit of internally chosen goals. We have set the conditions which predict various classes of behaviors—self-directing behaviors, sensitivity to realities within and without, flexible adaptiveness—which are by their very nature unpredictable in their specifics. Our recent research (*17*) indicates that our predictions are to a significant degree corroborated, and our commitment to the scientific method causes us to believe that more effective means of achieving these goals may be realized.

Research exists in other fields—industry, education, group dynamics—which seems to support our own findings. I believe it may be conservatively stated that scientific progress has been made in identifying those conditions in an interpersonal relationship which, if they exist in *B*, are followed in *A* by greater maturity in behavior, less dependence on others, an increase in expressiveness as a person, an increase in variability, flexibility and effectiveness of adaptation, an increase in self-responsibility and self-direction. And, quite in contrast to the concern expressed by some, we do not find that the creatively adaptive behavior which results from such self-directed variability of expression is a "happy accident" which occurs in "chaos." Rather, the individual who is open to his experience, and self-directing, is harmonious not chaotic, ingenious rather than random, as he orders his responses imaginatively toward the achievement of his own purposes. His creative actions are no more a "happy accident" than was Einstein's development of the theory of relativity.

Thus we find ourselves in fundamental agreement with John Dewey's statement: "Science has made its way by releasing, not by suppressing, the elements of variation, of invention and innovation, of novel creation in individuals" (*18*). Progress in personal life and in group living is, we believe, made in the same way.

Possible Concept of the Control of Human Behavior

It is quite clear that the point of view I am expressing is in sharp contrast to the usual conception of the relationship of the behavioral sciences to the control of human behavior. In order to make this contrast even more blunt, I will state this possibility in paragraphs parallel to those used before.

1) It is possible for us to choose to value man as a self-actualizing process of becoming; to value creativity, and the process by which knowledge becomes self-transcending.

2) We can proceed, by the methods of science, to discover the conditions which necessarily precede these processes and, through continuing experimentation, to discover better means of achieving these purposes.

3) It is possible for individuals or groups to set these conditions, with a minimum of power or control. According to present knowledge, the only authority necessary is the authority to establish certain qualities of interpersonal relationship.

4) Exposed to these conditions, present knowledge suggests that individuals become more self-responsible, make progress in self-actualization, become more flexible, and become more creatively adaptive.

5) Thus such an initial choice would inaugurate the beginnings of a social system or subsystem in which values, knowledge, adaptive skills, and even the concept of science would be continually changing and self-transcending. The emphasis would be upon man as a process of becoming.

I believe it is clear that such a view as I have been describing does not lead to any definable utopia. It would be impossible to predict its final outcome. It involves a step-by-step development, based on a continuing subjective choice of purposes, which are implemented by the behavioral sciences. It is in the direction of the "open society," as that term has been defined by Popper (*19*), where individuals carry responsibility for personal decisions. It is at the opposite pole from his concept of the closed society, of which *Walden Two* would be an example.

I trust it is also evident that the whole emphasis is on process, not on end-states of being. I am suggesting that it is by choosing to value certain qualitative elements of the process of becoming that we can find a pathway toward the open society.

The Choice

It is my hope that we have helped to clarify the range of choice which will lie before us and our children in regard to the behavioral sciences. We can choose to use our growing knowledge to enslave people in ways never dreamed of before, depersonalizing them, controlling them by means so carefully selected that they will perhaps never be aware of their loss of personhood. We can choose to utilize our scientific knowledge to make men happy, well-behaved, and productive, as Skinner earlier suggested. Or we can insure that each person learns all the syllabus which we select and set before him, as Skinner now suggests. Or at the other end of the spectrum of choice we can choose to use the behavioral sciences in ways which will free, not control; which will bring about constructive variability, not conformity; which will develop creativity, not contentment; which will facilitate each person in his self-directed process of becoming; which will aid individuals, groups, and even the concept of science to become self-transcending in freshly adaptive ways of meeting life and its problems. The choice is up to us, and, the human race being what it is, we are likely to stumble about, making at times some nearly disastrous value choices and at other times highly constructive ones.

I am aware that to some, this setting forth of a choice is unrealistic, because a choice of values is regarded as not possible. Skinner has stated: "Man's vaunted creative powers . . . his capacity to choose and our right to hold him responsible for his choice—none of these is conspicuous in this new self-portrait (provided by science). Man, we once believed, was free to express himself in art, music, and literature, to inquire into nature, to seek salvation in his own way. He could initiate action and make spontaneous and capricious changes of course. . . . But science insists that action is initiated by forces impinging upon the individual, and that caprice is only another name for behavior for which we have not yet found a cause" (*12*, pp. 52–53).

I can understand this point of view, but I believe that it avoids looking at the great paradox of behavioral science. Behavior, when it is examined scientifically, is surely best understood as determined by prior causation. This is one great fact of science. But responsible personal choice, which is the most essential element in being a person, which is the core experience in psychotherapy, which exists prior to any scientific endeavor, is an equally prominent fact in our lives. To deny the experience of responsible choice is, to me, as restricted a view as to deny the possibility of a behavioral science. That these two important elements of our experience appear to be in contradiction has perhaps the same significance as the contradiction between the wave theory and the corpuscular theory of light, both of which can be shown to be true, even though incompatible. We cannot profitably deny our subjective life, any more than we can deny the objective description of that life.

In conclusion then, it is my contention that science cannot come into being without a personal choice of the values we wish to achieve. And these values we choose to implement will forever lie outside of the science which implements them; the goals we select, the purposes we wish to follow, must always be outside of the science which achieves them. To me this has the encouraging meaning that the human person, with his capacity of subjective choice, can and will always exist, separate from and prior to any of his scientific undertakings. Unless as individuals and groups we choose to relinquish our capacity of subjective choice, we will always remain persons, not simply pawns of a self-created science.

III [Skinner]

I cannot quite agree that the practice of science *requires* a prior decision about goals or a prior choice of values. The metallurgist can study the properties of steel and the engineer can design a bridge without raising the question of whether a bridge is to be built. But such questions are certainly frequently raised and tentatively answered. Rogers wants to call the answers "subjective choices of values." To me, such an expression suggests that we have had to abandon more rigorous scientific practices in order to talk about our own behavior. In the experimental analysis of other organisms I would use other terms, and I shall try to do so here. Any list of values is a list of reinforcers—conditioned or otherwise. We are so constituted that under certain circumstances food, water, sexual contact, and so on, will make any behavior which produces them more likely to occur again. Other things may acquire this power. We do not need to say that an organism chooses to eat rather than to starve. If you answer that it is a very different thing when a man chooses to starve, I am only too happy to agree. If it were not so, we should have cleared up the question of choice long ago. An organism can be reinforced by—can be made to "choose"—almost any given state of affairs.

Rogers is concerned with choices that involve multiple and usually conflicting consequences. I have dealt with some of these elsewhere (*20*) in an analysis of self-control. Shall I eat these delicious strawberries today if I will then suffer an annoying rash tomorrow? The decision I am to make used to be assigned to the province of ethics. But we are now studying similar combinations of positive and negative consequences, as well as collateral conditions which affect the result, in the laboratory. Even a pigeon can be taught some measure of self-control! And this work helps us to understand the operation of certain formulas—among them value judgments—which folk-wisdom, religion, and psychotherapy have advanced in the interests of self-discipline. The observable effect of any statement of value is to alter the relative effectiveness of reinforcers. We may no longer enjoy the strawberries for thinking about the

rash. If rashes are made sufficiently shameful, illegal, sinful, maladjusted, or unwise, we may glow with satisfaction as we push the strawberries aside in a grandiose avoidance response which would bring a smile to the lips of Murray Sidman.

People behave in ways which, as we say, conform to ethical, governmental, or religious patterns because they are reinforced for doing so. The resulting behavior may have far-reaching consequences for the survival of the pattern to which it conforms. And whether we like it or not, survival is the ultimate criterion. This is where, it seems to me, science can help—not in choosing a goal, but in enabling us to predict the survival value of cultural practices. Man has too long tried to get the kind of world he wants by glorifying some brand of immediate reinforcement. As science points up more and more of the remoter consequences, he may begin to work to strengthen behavior, not in a slavish devotion to a chosen value, but with respect to the ultimate survival of mankind. Do not ask me why I want mankind to survive. I can tell you why only in the sense in which the physiologist can tell you why I want to breathe. Once the relation between a given step and the survival of my group has been pointed out, I will take that step. And it is the business of science to point out just such relations.

The values I have occasionally recommended (and Rogers has not led me to recant) are transitional. Other things being equal, I am betting on the group whose practices make for healthy, happy, secure, productive, and creative people. And I insist that the values recommended by Rogers are transitional, too, for I can ask him the same kind of question. Man as a process of becoming—*what*? Self-actualization—for what? Inner control is no more a goal than external.

What Rogers seems to me to be proposing, both here and elsewhere (*1*), is this: Let us use our increasing power of control to create individuals who will not need and perhaps will no longer respond to control. Let us solve the problem of our power by renouncing it. At first blush this seems as implausible as a benevolent despot. Yet power has occasionally been foresworn. A nation has burned its Reichstag, rich men have given away their wealth, beautiful women have become ugly hermits in the desert, and psychotherapists have become nondirective. When this happens, I look to other possible reinforcements for a plausible explanation. A people relinquish democratic power when a tyrant promises them the earth. Rich men give away wealth to escape the accusing finger of their fellowmen. A woman destroys her beauty in the hope of salvation. And a psychotherapist relinquishes control because he can thus help his client more effectively.

The solution that Rogers is suggesting is thus understandable. But is he correctly interpreting the result? What evidence is there that a client ever becomes truly *self*-directing? What evidence is there that he ever makes a truly *inner* choice of ideal or goal? Even though the therapist does not do the choosing, even though he encourages "self-actualization"—he is not out of control as long as he holds himself ready to step in when occasion demands—when, for example, the client chooses the goal of becoming a more accomplished liar or murdering his boss. But supposing the therapist does withdraw completely or is no longer necessary—what about all the other forces acting upon the client? Is the self-chosen goal independent of his early ethical and religious training? of the folk-wisdom of his group? of the opinions and attitudes of others who are important to him? Surely not. The therapeutic situation is only a small part of the world of the client. From the therapist's point of view it may appear to be possible to relinquish control. But the control passes, not to a "self," but to forces in other parts of the client's world. The solution of the therapist's problem of power cannot be *our* solution, for we must consider *all* the forces acting upon the individual.

The child who must be prodded and nagged is something less than a fully developed human being. We want to see him hurrying to his appointment, not because each step is taken in response to verbal reminders from his mother, but because certain temporal contigencies, in which dawdling has been punished and hurrying reinforced, have worked a change in his behavior. Call this a state of better organization, a greater sensitivity to reality, or what you will. The plain fact is that the child passes from a temporary verbal control exercised by his parents to control by certain inexorable features of the environment. I should suppose that something of the same sort happens in successful psychotherapy. Rogers seems to me to be saying this: Let us put an end, as quickly as possible, to any pattern of master-and-slave, to any direct obedience to command, to the submissive following of suggestions. Let the individual be free to adjust himself to more rewarding features of the world about him. In the end, let his teachers and counselors "wither away," like the Marxist state. I not only agree with this as a useful ideal, I have constructed a fanciful world to demonstrate its advantages. It saddens me to hear Rogers say that "at a deep philosophic level" *Walden Two* and George Orwell's *1984* "seem indistinguishable." They could scarcely be more unlike—at any level. The book *1984* is a picture of immediate aversive control for vicious selfish purposes. The founder of *Walden Two*, on the other hand, has built a community in which neither he nor any other person exerts any *current* control. His achievement lay in his original *plan*, and when he boasts of this ("It is enough to satisfy the thirstiest tyrant") we do not fear him but only pity him for his weakness.

Another critic of *Walden Two*, Andrew Hacker (*2*), has discussed this point in considering the bearing of mass conditioning upon the liberal notion of autonomous man. In drawing certain parallels between the Grand Inquisition passage in Dostoevsky's *Brothers Karamazov*, Huxley's *Brave New World*, and *Walden Two*, he attempts to set up a distinction to be drawn in any society between conditioners and conditioned. He assumes that "the conditioner can be said to be autonomous in the traditional liberal sense." But then he notes: "Of course the conditioner has been conditioned. But he has not been conditioned by the conscious manipulation of another *person*." But how does this affect the resulting behavior? Can we not soon forget the origins of the "artificial" diamond which is identical with the real thing? Whether it is an "accidental" cultural pattern, such as is said to have produced the founder of *Walden Two*, or the engineered environment which is about to produce his successors, we are dealing with sets of conditions generating human behavior which will ultimately be measured by their contribution to the strength of the group. We look to the future, not the past, for the test of "goodness" or acceptability.

If we are worthy of our democratic heritage we shall, of course, be ready to resist any tyrannical use of science for immediate or selfish purposes. But if we value the achievements and goals of democracy we must not refuse to apply science to the design and construction of cultural patterns, even though we may then find ourselves in some sense in the position of controllers. Fear of control, generalized beyond any warrant, has led to a misinterpretation of valid practices and the blind rejection of intelligent planning for a better way of life. In terms which I trust Rogers will approve, in conquering this fear we shall become more mature and better organized and shall, thus, more fully actualize ourselves as human beings.

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EEG, Consciousness, and Sleep

Charles W. Simon and William H. Emmons

Since the discovery of brain waves, considerable effort has been made to link this physiological activity to psychological correlates. After more than a quarter of a century of research, this hope has been only partially realized. Although clinicians and neurologists have found the EEG useful as a diagnostic tool, its contribution to the psychologist working with normal individuals has been slight (*1*).

The state of wakefulness and sleep of a normal individual, however, has been related successfully to changes in the EEG (*2*). A number of investigators have found that varying depths of sleep, as measured by the length or intensity of tones required to awaken the subject, are related to increases in amplitude and decreases in frequencies of delta-type (*3*) electroencephalographic patterns. When subjects were awake, alpha rhythms (*4*) could generally be detected (*5, 6*).

This article relates specific EEG patterns along the continuum between waking and deep sleep with more complex behaviors associated with degrees of consciousness and unconsciousness. It would be foolish to belabor a precise definition of consciousness. Two measures that are believed to be highly correlated with the "degree of consciousness" in normal and motivated individuals are (i) the ability to recognize and report the occurrence of particular stimuli to which they have been instructed to attend and (ii) the ability to remember and later recall these stimuli.

Consciousness refers to stages of the waking state during which degrees of awareness of external stimuli occur and to the transition state during which internal stimuli—that is, dreams—occur and are recalled. Unconsciousness refers to the state in which various stages of sleep occur. This article emphasizes the investigation of the waking end of the continuum.

Materials and Methods

Twenty-one normal, adult male subjects were selected on the basis of IQ (average or above) and a monopolar, occipital EEG showing a continuous alpha rhythm when they were awake and resting with their eyes closed (*7*).

Subjects were pretested to see whether they knew the answers to 96 factual questions on history, sports, science, and the like. They then retired to soundproof, air-conditioned booths for a normal 8-hour night's sleep. The same questions along with the correct answers were played one at a time at 5-minute intervals during the night. Continuously throughout this entire period, monopolar electroencephalographic recordings were made from each subject's right occiput and vertex. A pen marker showed the exact sections of electroencephalographic record during which the questions and the answers occurred.

Subjects were asked to call out their names immediately if they heard the answer to any question. After the 8-hour training period, all subjects were awakened and given the questions again and were tested to determine which of the answers not known previously could now be recalled.

Alpha as an Index of Consciousness

The positive relation between alpha and consciousness has been noted by a number of investigators (*5, 6*). In the present experiment, the period between awakefulness and sleep was extended sufficiently to provide a means of studying the relationship between the quantity and quality of alpha and variations in consciousness, as measured by responding and recalling.

Figure 1 shows sample EEG patterns from the right occiput along with their corresponding measures of consciousness. The letters assigned to the sleep levels correspond quite closely to those used by other investigators (*6*). Figure 1 illustrates that as the quality and quantity of alpha increases, so does the probability that a stimulus will be reported heard when it occurs and correctly recalled later.

Within level O, a slight reduction in the amplitude of the continuous waking alpha before going to sleep and after awakening from sleep was related to a similar decrease in the probability that an appropriate response would be made. As the percentage of alpha continues to decrease in quantity and amplitude in levels A+ and A, there is a corresponding decrease in the probability of responding or recalling. As the individual becomes very drowsy and level A– patterns are observed, the cyclical activity still remains, although it is approximately 2 cycles per second slower than the subject's normal waking alpha rhythm. These waves fall within the alpha frequencies, and recall still has a relatively low probability of occurrence, as does an immediate response.

For the sake of completeness, the obvious should be emphasized. Lack of alpha does not guarantee lack of consciousness. Alpha may disappear during

Dr. Simon is a member of the technical staff of Hughes Weapon Systems Development Laboratories, Culver City, Calif., and Mr. Emmons is associated with the RAND Corporation, Santa Monica, Calif.

excitement or when concentration takes place (*8*). Furthermore, many normal individuals show little or no observable waking alpha rhythm. Therefore, although alpha may be used to identify consciousness, lack of consciousness, or unconsciousness, must be identified in some other manner.

Delta as an Index of Unconsciousness

In Fig. 2 are shown electroencephalographic patterns—the delta waves—associated with sleep or unconsciousness. No alpha activity was observed within 30 seconds on either side of the answer period. In all levels when this delta activity predominated, subjects neither responded to nor recalled material presented to them; subjects were unconscious and asleep when the patterns in Fig. 2 were observed (*9*).

Stimulus Effects

Unlike the EEG patterns shown in Figs. 1 and 2, approximately 14 percent of the patterns showed marked alterations following the onset of stimulation. Therefore, when we were searching for the presence of alpha frequencies, the record up to 30 seconds on either side of the answer period was included.

In Fig. 3, EEG patterns showing the effects of auditory stimulation on records from both the occiput and the vertex are presented, along with the percentage of items reported heard and correctly recalled when these patterns occurred. From Fig. 3 it can be seen that (i) the ability of subjects to recall and to give immediate responses corresponds to the quality and quantity of alpha frequencies in the vicinity of stimulation and (ii) when alpha and delta frequencies are mixed, the probability of recall is related to whichever of these components predominates. These relations between consciousness and the EEG correspond to those observed with patterns showing no stimulus effects.

In levels A+, A−, and B the alpha frequencies predominated, and the subjects were able to respond and recall to some degree. In levels C and D, the delta waves predominated, and neither criteria of consciousness occurred. In the two placements used, alpha activity is more evident in the occipital tracings, while delta is more prevalent in the vertex recordings.

With pattern *AbA*, the continuous alpha rhythm was blocked at the onset of stimulation only to return when stimulation stopped. This is the classical alpha block (*10*) which differs from the condition where the absence of alpha is associated with deeper drowsy stages in that the disappearance of alpha coincides with stimulation. Pattern *bA* illustrates the situation in which the stimulation is followed by the subject's rapid awakening. With presence of the waking alpha rhythm immediately after stimulation, recall still occurs about 50 percent of the time (*11*). Recall decreases in pattern *A* when stimulation causes a recurrence of alpha but of mixed frequencies between 8 and 12 cycles per second of poorer quality than the waking alpha rhythm.

The percentage of responses drops still further in pattern *Ab* when the subject wakens to the question but falls back to sleep. Alpha is seen only prior to the period when the answer is given.

In the lower section of Fig. 3, the delta waves predominate in patterns *Ds* and *D*. Their amplitude increasing during stimulation. Some of these patterns have been described as K-complexers (*12*). No recalling or responding occurred to items presented at these times.

In the borderline patterns *A/d* and *A/D*, stimulation led to a mixture of alpha and delta. These followed the principle stated earlier; that is, when alpha and delta were mixed, the probability of recall was related to whichever frequencies predominated.

The one exception to the alpha-delta principle occurred with pattern *X* when the stimulus affected the null state patterns of level B. Probability of recall

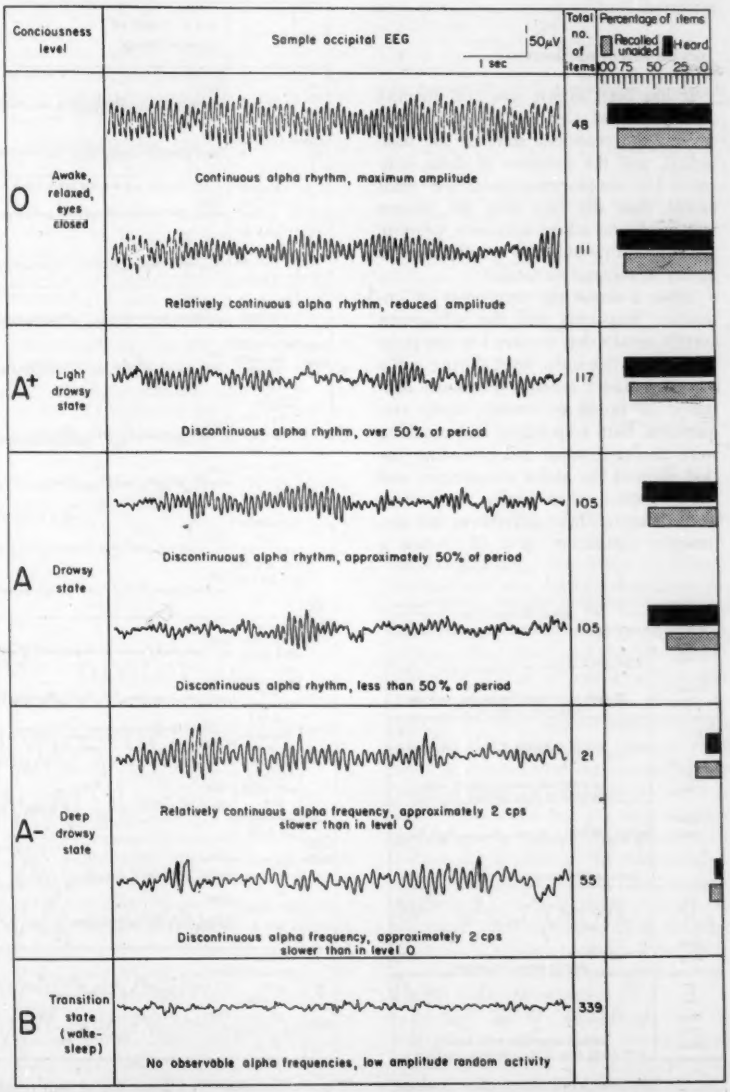

Fig. 1. Percentage of items heard and recalled during the appearance of waking electroencephalograms.

and responses was equivalent to the mixed-wave patterns, although no alpha or delta frequencies were observed; this particular pattern has been associated by other researchers with arousal (*13*).

It should be pointed out that these different patterns did not always occur alone. In fact, when they occurred at all, they generally appeared sequentially as the subject was roused by the stimulation from sleep to wakefulness. At times they would begin when the question was given; at times, when the answer was given. The order of appearances of these patterns corresponded to the shift from the bottom of Fig. 3 upward. Although the order was always the same, patterns were often skipped, apparently depending on the speed at which awakening occurred.

Inertia Effect of Sleep

It has been shown how the quantity and quality of alpha in a short record reflects the conscious state of the individual, and the presence of delta indicates his unconscious state. To what extent does the fact that the subject was awake or asleep sometime prior to the stimulus period have an effect on his ability to respond or recall?

Table 1 shows the percentage of immediate responses and the subsequent correct recalls that occurred at any sleep level when the sleep level during a 60-second period *5 minutes previously* contained or failed to contain alpha frequencies. Both responding and recalling were hindered when the preceding period showed no alpha frequencies and were favored when alpha frequencies were present. These differences are statistically significant ($p < .05$) when a chi-square test was applied to the combined data; the only exception was level 0, "Items reported heard" ($p < .20$).

The foregoing results led to the formulation of the "inertia effect" principle. This principle states: During the presence of any waking EEG pattern, subjects who have recently been asleep tend to show a lower probability of responding or recalling than those who have been awake previously.

Alpha, Movement, and Consciousness

Can the movement of a subject during sleep be used as a criterion of conscious-

Table 1. Immediate responses and subsequent recalls with and without alpha frequencies during a 60-second EEG record 5 minutes earlier.

Current sleep level	Items reported heard (%)		Items recalled correctly (%)	
	Alpha present	No alpha	Alpha present	No alpha
O	89	70	84	27
A+	83	58	79	38
A	72	43	59	42
A−	43	26	40	19
B	10	2	8	3

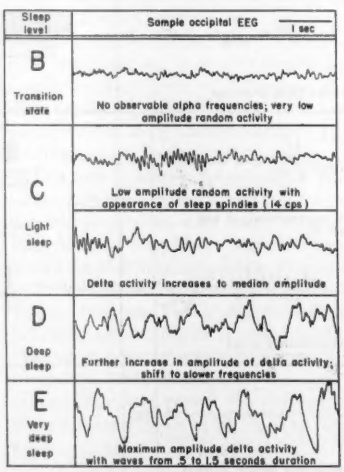

Fig. 2. Electroencephalographic patterns during sleep.

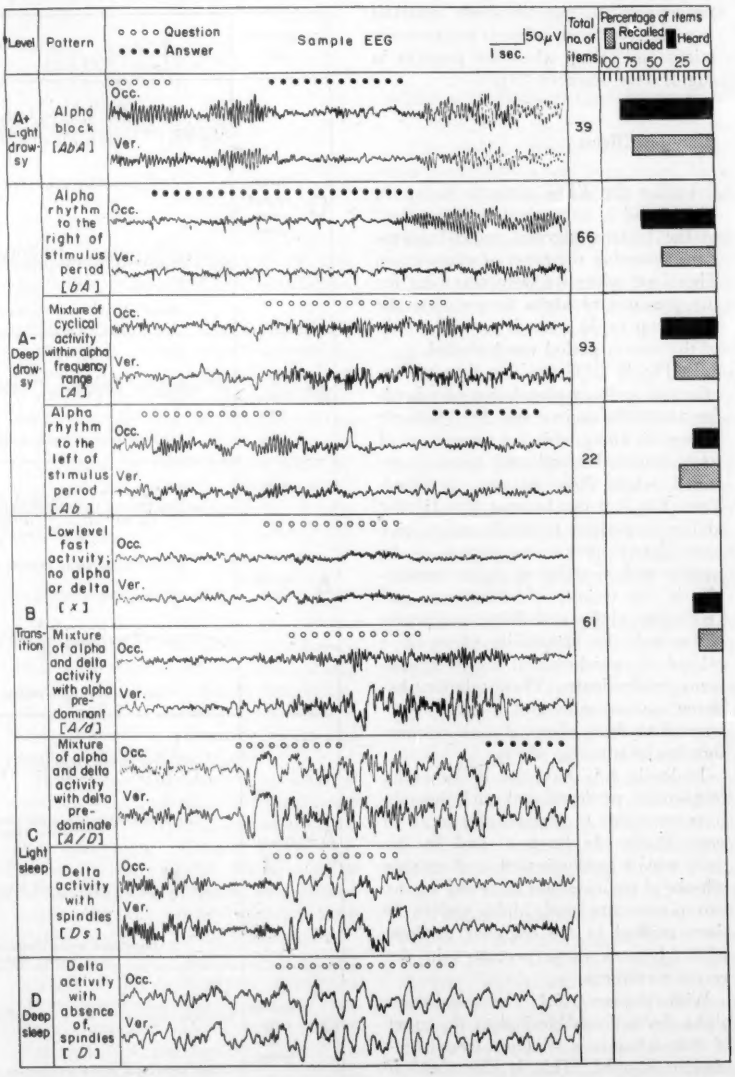

Fig. 3. Percentage of items heard and recalled during the appearance of electroencephalographic patterns modified by stimulation.

ness? What is the relation between alpha, movement, and consciousness as measured by immediate responses and subsequent recall? Table 2 shows the percentage reported heard and the percentage recalled of items that occurred during movements. Since cortical potentials were partially or completely obscured by movement artifacts, it was impossible to assign these items to specific sleep levels. However, the presence or absence of alpha in the vicinity could be detected. When alpha rhythms were found during or at the end of the movement, hearing and recalling tended to be high; when no alpha was observed, subjects heard and recalled practically nothing. These results corresponded with those of alpha and nonalpha periods without movement. Thus, two conclusions can be drawn: (i) it is possible to have movement without the apparent presence of the waking alpha; (ii) the presence of alpha and not movement is the critical criterion for conscious responses.

Table 2. Items heard and recalled with and without movement and alpha frequencies.

	Reported heard (%)		Recalled correctly (%)	
	Alpha	Nonalpha	Alpha	Nonalpha
Move	57	8	35	2
No movement*	63	3	56	3

* Where cortical patterns were not obscured by movement artifacts, the alpha category corresponds to levels O, A+, A, A− and the nonalpha category to levels B, C, D, and E, including all stimulus effects.

Application

A systematic change has been found in the electroencephalographic patterns of alpha-dominated subjects as they go from a state of relaxed wakefulness to deep sleep.

The relation of alpha to wakefulness and delta to sleep and the probability of responding and recalling only when alpha is present negates popular contentions that learning during sleep is possible. This psychological problem has been discussed more thoroughly in other papers (*7, 9, 14*).

One important practical application of the information in this paper is in the study of factors affecting sleep and rest. In addition to the more classical measure of time (or length of sleep), electroencephalographic patterns can provide a means of continuously measuring depth of sleep without disturbing the subject. Using a two-dimensional measure—length and depth—is a more sophisticated approach to certain problems of sleep and rest and can be expected to yield more satisfactory conclusions. Such problems have important implications for both military and civilian use.

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F. Soddy, Interpreter of Atomic Structure

Frederick Soddy is best known as Ernest Rutherford's young coworker at McGill University at the beginning of this century, when the disintegration theory of radioactivity was put forward, and as the young lecturer at Glasgow who saw most clearly that the heaviest chemical elements have isotopes. Some who knew him in his later life knew him only as one who held heterodox views on economics and politics and as a "crank" on the subject of monetary policy. Those who knew him personally knew him as a fine specimen of English manhood, a good husband, a generous helper of those less well off than himself, a lover of truth (including unpleasant truth) with an original outlook on many of life's problems. He was too shy or too aloof to shine in society and much too earnest and serious minded to suffer anything approaching a fool gladly.

In appearance Soddy was a fairhaired, 6-foot Saxon type with a fine head and regular features. His voice was high-pitched and very southern English in accent. He was born on the south coast at Eastbourne in 1877, went to an English public school there and then to Oxford, where he graduated with first-class honors in chemistry in 1898, thence to McGill till 1902. After a year in London with Sir William Ramsay (during which time he showed that Rutherford's alpha particle from radium was an atom of helium) he became a lecturer at Glasgow, where he remained for 10 years till he became a professor at Aberdeen. In 1919, World War I being over, he went to Oxford as a professor and remained there till 1936, retiring at 59 to Brighton on the south coast 15 miles from where he was born. And there he died and was buried last September.

It was Soddy's misfortune that he never had a wide circle of friends among his fellow-scientists or was *persona grata* with the large staff of fellows and lecturers in chemistry at Oxford. If blame

there must be for this, it may be ascribed to his upbringing. He was the youngest of seven sons of a London businessman resident in Eastbourne who was a keen nonconformist in religion. He lost his mother before he was 2 years old. A stern upbringing in a house of older brothers, housekeepers, and maids turned him into a solitary, an "original," and the doctrine that truth is the chief thing that matters in life tended to make him too serious and to look on life's problems with too painful an eye. Much of his habitual seriousness and earnestness of purpose was modified when he married Winifred, the daughter of the industrialist Sir George Beilby, F.R.S., in 1908. She made him a charming wife. She was the best type of hostess. She and her husband always got on excellently together. However blasting might be Soddy's invective against persons or things at times, there was never anything of that between the married pair. When she died in 1936 Soddy immediately made plans for resigning his chair and leaving Oxford, so heartbroken was he. They had no children.

In 1900, Rutherford at 29 in the physics department of McGill and Soddy at 23 in the chemistry department were admirably placed for bringing light into the darkness that enshrouded the subject of radioactivity then. The Curies and other workers on radium and thorium had really no idea what was taking place. Rutherford and Soddy, by their joint work and their remarkable insight, soon showed that what was occurring among the very heaviest of the elements was a spontaneous transformation of one element into another, a change, moreover, that could not be modified in any way by any physical or chemical method then available. These heavy elements lacked no properties that other elements possessed; they possessed in addition this remarkable and exciting property of radioactivity. Although Rutherford was the greater man of the two, as is shown by his subsequent work, there is no doubt that equal credit must be given to Soddy for this discovery.

Soddy's second big discovery was the existence and importance of isotopes among the radioactive elements; it came in 1913. All elements above mercury in the periodic classification are radioactive, but there are only 12 places for them in the classification. The researches of many people had shown, however, by 1913 that there were 40 or 50 different radioactive elements. Soddy showed clearly that all of these, as regards their chemical properties, could be properly placed in ten of the 12 places available. Those positioned by their chemical properties in one particular place of the classification were appropriately dubbed isotopes by him.

In later life and especially during his second Oxford period, when he was professor of inorganic and physical chemistry and fellow of Exeter College, Soddy turned his attention more and more to economic problems and made no further big discoveries in chemistry. The lessons of the great war of 1914–19 were, of course, partly responsible for this change. Its waste, its futility, and its hatreds angered him. He began to believe that the scientist ought to contribute more to the body politic than his discoveries, important though these undoubtedly are. He felt that the future is more worth working for than the present, because there is so much more of it. He felt, too, that, if science could only get going properly, the miserable poverty that assailed so many of us in Europe could be easily overcome.

Like Emerson he realized that all the good things on the bread-and-butter side of life are on the highway. The difficulty is to get them into the cottage homes at a reasonable cost. Nature and the factory —so far advanced was applied science— could get everything made at reasonable, nay at trifling, cost. The great hindrance to this, according to Soddy, was the monetary policy of England and other countries. It and the banks and the middleman were robbing all of us of quite half the benefits that science could easily give us. He believed that salvation lay in schemes like the Douglas scheme or Social Credit.

His fanatical devotion to schemes of this sort, derided by the orthodox economists, and exposed by the logician, was surprising to many who knew him first as a pioneer in chemical science and the winner of a Nobel prize, and they were shocked—many of them—when he referred to our monetary system as a private, upstart, and frustrating affair, which was a hell's brew at bottom. No doubt, he exaggerated when he described banks as morally no better than disorderly houses or gambling hells. He saw the Royal Society of London—surely one of the goodliest societies on earth—as an unofficial part of the English Civil Service, concerned more with the knowledge that preserved or strengthened the *status quo* of society than with following truth wherever it may lead us. Three of the books he wrote on these subjects were *Cartesian Economics* (1922), *Wealth, Virtual Wealth and Debt* (1926), and *The Arch-Enemy of Economic Freedom* (1943).

Although Soddy had not the wide popularity of a Ramsay or a Rutherford, those who knew him well were very fond of him. To them he showed his best side. He was on occasion generosity itself, and many of his pupils can bless him for the helping hand he gave them when finance or fortune was at a low ebb.

ALEXANDER S. RUSSELL
Christ Church, Oxford England

News of Science

NSF Report on Federal Budget for Research

The Federal Government during fiscal year 1956 spent an estimated $2.4 billion for scientific research and development activities, a 7 percent increase over 1955. For the current fiscal year, about $2.7 billion will be spent, 12 percent more than in 1956. These data are contained in a new National Science Foundation report, *Federal Funds for Science, V*.

The report, the fifth in a series, analyzes for the fiscal years 1955, 1956, and 1957, the Federal scientific research and development budgets according to administering agencies, scientific fields, and character of the research. For the first time, data are included that indicate the distribution of Federal funds among the organizations performing Government research.

Approximately 85 to 90 percent of the total funds are for the conduct of research and development; the remainder, for the expansion of the research and development plant and facilities.

Twenty-five agencies of the executive branch administer research and development funds, with individual agencies spending from less than $50,000 a year to approximately $2 billion. However, 99 percent of the funds were spent by eight agencies: Department of Defense; Atomic Energy Commission; Department of Health, Education, and Welfare; Department of Agriculture; National Advisory Committee for Aeronautics; Department of Interior; Department of Commerce; and the National Science Foundation.

By far the greater part of the Federal research budget is devoted to applied research and development. The proportion spent for basic research although comparatively small, is increasing. In fiscal year 1957, basic research will account for about 9 percent of the research budget.

The Federal research dollar during fiscal year 1956 was divided in the following manner: 47 cents for research performed in the Government's own laboratories; 38 cents for research by profit organizations; 13 cents for educational institutions; and the remainder by other organizations. These figures include obligations for research centers which are Government-financed research installations managed for the Government by private organizations. These research centers account for approximately 15 cents of the research dollar.

Physical sciences, including engineering, account for about 87 percent of Government research funds, the life sciences for 11 percent, and the social sciences for 2 percent. A copy of *Federal Funds for Science, V* may be obtained for 35 cents from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D. C.

N.Y. Botanical Garden Laboratory

A new $1-million laboratory was dedicated recently at the New York Botanical Garden, of which William J. Robbins, botanist, is director. The laboratory includes 32 research rooms with elaborate control of light, temperature, and humidity. The projects to be studied at the new facility will deal with plant nutrition and propagation, abnormal plant growth, antibiotics and antiviral substances associated with plants, and plant diseases. Run-of-the-mill problems will be investigated, too; for example, whether or not natural gas is harmful to vegetation, and whether or not the compost pile can be a source of plant diseases and plant-feeding insects. The city of New York contributed $185,000 toward the cost of the new laboratory and will pay some of the administrative and operating expenses.

ICAO Standard Atmosphere

A new extension to the 20-kilometer International Civil Aviation Organization standard atmosphere (the accepted U.S. standard) is being adopted by about 23 United States scientific and engineering organizations; this extension provides tables of atmospheric parameters up to 300 kilometers. The new standard atmosphere has a temperature of about 217°K at 25 kilometers, 283°K at 47 kilometers, 197°K at 75 kilometers, and then increases more or less continuously above 90 kilometers to reach a temperature of more than 1000°K at 300 kilometers.

Because of their great need for such tables in their high-altitude research programs, the 23 organizations met in November 1953 to seek agreement on a single representation of the atmosphere compatible with the best available data. The U.S. Weather Bureau of the Department of Commerce and the Geophysics Research Directorate, Air Force Cambridge Research Center of the Air Research and Development Command, cosponsored this movement. For further information, write to the committee on Extension to the Standard Atmosphere, Geophysics Research Directorate, 415 Summer St., Boston 10, Mass.

Australian Water Conservation Method

At the United Nations Educational, Scientific and Cultural Organization symposium on climatology that took place recently in Canberra, Australia, Richard G. Casey, Australian minister in charge of industrial scientific research, reported that Australia would make freely available to other countries its new method of using cetyl alcohol, a chemical extracted from whale oil, to limit evaporation from reservoirs and dams. Cetyl alcohol, which is invisible, tasteless, and harmless to animal life, lays a film over the surface of water and keeps it from evaporating. It does not prevent oxygen from entering and so keeps the water fresh. Tests during the last 2 years in dams and reservoirs have shown a reduction in evaporation of between 20 and 70 percent.

Physics Institute Expansion Program

The American Institute of Physics is embarking on a fund-raising campaign for $500,000 to finance a new headquarters building in New York, expand the publishing of technical journals, and attract more well-qualified young men and women to the profession of physics. The AIP is an association of five professional societies with 17,000 members.

The present AIP headquarters building at 57 E. 55 St. is inadequate, and another building at 335 E. 45 St. has been purchased for $280,000. This structure, which is not far from the United Nations Plaza, is four stories high. After remodeling, it will provide three times the working space now available. The AIP headquarters staff has grown from 25 to 60 since the present building was first occupied. The institute's membership has more than doubled, and the

number of journal pages published annually has increased more than four times.

Conversion of the new headquarters will start in December and operations will be transferred by 1 June 1957. Paul E. Klopsteg, associate director of the National Science Foundation and professor emeritus of applied science at Northwestern University, has been appointed chairman of the American Institute of Physics Development Fund Committee.

The half-million dollars being sought will be allocated as follows: $250,000 for conversion of the building on 45 St. for use as AIP headquarters; $200,000 for new and improved technical journals of physics to speed the flow of research information; and $50,000 for more effective work to attract and train more and better physicists. Of the $500,000, $350,000 is being requested from industry and the remaining $150,000 from members of the AIP.

The publishing crisis facing the field of physics, and for which funds are being sought, arises from the fact that even though almost 19,000 pages appeared in journals published by the AIP last year, another 5000 pages are needed now to take care of reporting new research. These new pages may be added to existing journals or used as the basis for one or more new scientific journals. Examples of special fields in which the demand for more publication space is rising are chemical, mathematical, and fluid physics.

The shortage of physicists, perhaps one of the most critical in the scientific manpower field, will be attacked by a "future physicists fund." This fund will be used to publish and distribute vocational information and to work for improved teaching and educational facilities at all school and college levels.

Engineering Center To Be in New York

United Engineering Trustees, Inc., the joint corporate agency of the four major national engineering societies, has announced that a contract has been signed for preliminary architectural plans and studies for a new Enginering Center in New York, N.Y. The announcement is the first formal notice that all the four member societies of UET, and a fifth that is expected to become a member, had voted, through their governing boards, to remain in New York. Several other cities had tried to obtain the center.

The present 16-story quarters, known as the Engineering Societies Building, houses the four "founder societies"—the American Society of Civil Engineers, the American Institute of Mining, Metallurgical and Petroleum Engineers, the American Society of Mechanical Engineers, and the American Institute of Electrical Engineers. A fifth prospective founder society, the American Institute of Chemical Engineers will be included in the center project.

Occupants of the Engineering Societies Building include the Engineers Joint Council, which is composed of ten major national engineering societies, including the five in the founder group. It is believed that preliminary plans for the center will be completed early next year.

Darwin Centennial Expedition

The Darwin Anniversary Committee, Inc., has announced that Charles Darwin's historic round-the-world trip, which helped him formulate his theory of evolution, will be retraced in 1958. The year 1958 was chosen because it will be the centennial of Darwin's presentation of his paper to the Linnaean Society in London outlining his theory of evolution. Julian S. Huxley, the biologist, is honorary cochairman of the planning committee. Lady Nora Barlow, a descendant of Darwin, is the other cochairman.

Darwin sailed in the British ship *Beagle* as official naturalist on a surveying trip. The expedition, which took place between 1831 and 1836, visited islands in the Atlantic, the coast of South America and adjacent islands, and islands of the western Pacific. The Darwin committee plans to cover the same areas in a year's time, using a 100 to 150-foot sailing ship with auxiliary engines.

On his trip Darwin studied native people and the flora and fauna of the areas. The modern voyage will compare ecologic conditions today with those of 125 years ago. The 1958 trip also will seek to determine if any species of flora and fauna are in danger of becoming extinct.

In the next few months about 20 scientists, both men and women, will be selected as Darwin fellows to sail on the expedition. Others probably will be flown to the research areas.

IGY Observations of the Ionosphere

A group of scientists has left the Boulder Laboratories of the National Bureau of Standards for a year in the Antarctic, where they will operate five widespread research stations. The men at each post will collect continuous data with the C-4 ionosphere recorder, an instrument that beams short pulses into the upper atmosphere, measuring the time required for them to travel there and back. A range of from 1 to 25 megacycles is covered in 15 seconds. A reading will be made automatically at least every 15 minutes.

The same kind of ionospheric observations will be carried out by scientists working simultaneously in more than 75 such stations located throughout the world as part of the International Geophysical Year program. By analyzing the total data, scientists expect to learn much about the height and characteristics of the upper air layers, which change from hour to hour, day to day, and season to season.

Supervising the ionospheric operations in the Antarctic is Hans J. Bengaard, former electrical engineer with the Danish Post and Telegraph Administration, who will also be in charge of the Little America station. His assistant will be Lt. Col. Carl O. Wyman, Philadelphia, Pa., retired communications and electronics officer in the U.S. Marine Corps.

Virgil Barden, Colorado Springs, Colo., formerly radio technician at the White Sands Proving Ground ionospheric station, will be at Byrd Base in charge of ionospheric observations.

John B. Brown, former physicist with E. I. duPont de Nemours, Inc., Bloomington, Del., will be at the Weddell sea station, where he will be accompanied by Donald Skidmore, former electronic engineer with the National Company, Malden, Mass.

Garth Stonehocker, previously physicist with the Glenn L. Martin Company, Baltimore, Md., and his assistant, Robert Long of Rochester, N.Y., will be at the Knox station.

William S. Hough, electronic engineer at the NBS Boulder Laboratories, will make observations from the South Pole station, located at the exact geographic bottom of the world.

PHS Aging Research Center

The U.S. Public Health Service has established a Center for Aging Research in the National Institutes of Health. G. Halsey Hunt, at present associate chief of the PHS Bureau of Medical Services, has been appointed director of the center, which has been set up to deal with the special health problems of the more than 12 million people in this country who are over the age of 65. By 1970, there will be more than 18 million such people.

The primary objective of the new program is to encourage and support additional research into the mechanisms involved in aging. The program will assist universities and other research institutions in establishing a broad research program that will bring the full range of biological, psychological, and social sciences to bear on the problem. A first activity will be to aid universities, medi-

cal schools, and other research organizations in establishing comprehensive research centers on aging, to be supported in part by NIH research grants.

Scientists in the News

ROBERT B. LIVINGSTON, professor of physiology and anatomy at the University of California School of Medicine, Los Angeles, is on a leave of absence to serve as scientific director of the combined basic research programs of the National Institute of Mental Health and the National Institute of Neurological Diseases and Blindness in Bethesda, Md. He assumed his new position on 15 Nov., when he succeeded SEYMOUR S. KETY, who had asked to be relieved as scientific director in order to devote full time to research in cerebral blood flow.

Livingston has just returned from Sweden, where a special NINDB fellowship enabled him to conduct studies on the influence of vestibular mechanisms on brain-stem and spinal motor systems. In recent years, Livingston has been particularly interested in the neurophysiological basis of behavior, and especially in the function of reticular formation in regulating sensory input to the brain.

TRYGVE RAMBERG, journalist for the Norwegian newspaper *Aftenposten* is touring this country for 6 months to observe U.S. science and to study science writing. He will spend January and February at the Northwestern University School of Journalism. Ramberg's trip is supported by the Joint Committee of Norwegian Research Councils.

JAMES H. DOOLITTLE, a vice president and director of the Shell Oil Company, has been named chairman of the National Advisory Committee for Aeronautics, the nation's principal aeronautical research agency. He succeeds J. C. HUNSAKER, chairman of the NACA for the past 15 years. LEONARD CARMICHAEL, secretary of the Smithsonian Institution, was reelected vice chairman.

KARL TERZAGHI, professor emeritus of civil engineering at Harvard University and a specialist in soil mechanics, has been appointed lecturer and research consultant in soil mechanics for the current academic year at Massachusetts Institute of Technology. Terzaghi taught at M.I.T. in the late 1920's as a special lecturer in soil mechanics, and as associate professor of foundation engineering. He developed at M.I.T. the first courses in soil mechanics to be given in the United States.

ALFRED O. C. NIER, chairman of the University of Minnesota's school of physics, and ARTHUR HOLMES, geologist at the University of Edinburgh, were honored at the recent meeting of the Geological Society of America for their outstanding work in the age determination of the earth. At special ceremonies, the society awarded its Arthur L. Day medal to Nier and its Penrose medal to Holmes. Because of illness Holmes was unable to attend the meeting.

The American Geographical Society awarded gold medals to the following men.

J. RUSSELL SMITH, professor emeritus of economic geography at Columbia University, received the Cullum geographical medal. He is the author of several books on North America that have become classics in their field.

RAOUL BLANCHARD, one of the elder statesmen of the French geographical school, long associated with the University of Grenoble, received the Charles P. Daly medal. Blanchard's work on the Alps is about to be completed with the appearance of the 12th volume this fall.

GEORGE McCUTCHEON McBRIDE, professor emeritus of geography at the University of California at Los Angeles, received the David Livingstone centenary medal. McBride is internationally recognized as an authority on problems of economic and cultural geography in Latin America.

The American Society for Horticultural Science has made the following awards:

GARTH A. CAHOON, Citrus Experiment Station, Riverside, Calif., and DUANE O. CRUMMETT, Los Angeles County Tuberculosis and Health Association, Los Angeles, Calif., received the Alex Laurie award in floriculture and ornamental horticulture.

E. M. RAHN, Delaware Agricultural Experiment Station, Newark, Del., received the Charles G. Woodbury award in raw products research.

G. H. HENDERSHOTT, c/o Arkansas Agricultural Mission, Ancon, Canal Zone, and LOWELL F. BAILEY, University of Arkansas, received the Joseph Harvey Gourley award in pomology.

A. A. PIRINGER, JR., University of Minnesota, and NEIL W. STUART, U.S. Department of Agriculture, Plant Industry Station, Beltsville, Md., received the Leonard H. Vaughan award in floriculture and ornamental horticulture.

H. C. MOHR, H. T. BLACKHURST, and E. R. JENSEN, Texas Agricultural Experiment Station, College Station, Tex., received the Leonard H. Vaughan award in vegetable crops.

ROBERT R. WHITE, professor of chemical engineering at the University of Michigan, will receive the 1956 professional progress award in chemical engineering of the American Institute of Chemical Engineers during the institute's annual meeting in Boston, Mass., 9–12 Dec. The award consists of $1000 and a certificate.

DAVID E. GREEN of the Institute for Enzyme Research, University of Wisconsin, has been invited by the Indian Science Congress and the Government of India to attend the annual meeting to be held in Calcutta 14–20 Jan. He will deliver a lecture before the Indian Society of Biological Chemists and will visit and lecture at various universities and research institutions throughout the country that are active in biochemical research and training.

R. G. WILSEY, D. H. STRANGWAYS, (deceased), and G. M. CORNEY have received the Coolidge award, which is given by the Society for Nondestructive Testing in cooperation with the General Electric Company's x-ray department. They were honored for their studies on the need for better methods of minimizing the amount of radiation received by personnel in the industrial x-ray field.

Wilsey, a physicist, has for the past 25 years been an instructor in the department of radiology at the University of Rochester School of Medicine and Dentistry. He was formerly with Kodak Research Laboratories and U.S. Bureau of Aircraft Production. Strangways was a physicist with Kodak Research Laboratories and, prior to that, an electrical engineer with Northern Electric Company, London, Ont. Corney has been physicist with Kodak Research Laboratories for the past 19 years.

RAY W. GRIM has joined the staff of the National Library of Medicine to serve in the capacity of executive officer in the office of the director. Grim, who has been in the U.S. Public Health Service since 1934, was formerly program management officer in the Division of Dental Public Health, Bureau of State Services.

HAROLD D. GREEN, director of the department of physiology and pharmacology at the Bowman Gray School of Medicine, delivered the 40th Mellon lecture before the Society for Biological Research of the University of Pittsburgh School of Medicine.

FRITZ LIPMANN has been appointed a member of the Rockefeller Institute. Lipmann, who received the Nobel prize for medicine and physiology in

1953, is now professor of biological chemistry at Harvard Medical School and head of the Biological Research Laboratory at Massachusetts General Hospital. He will take up residence in New York in the summer of 1957.

Work on the biochemical conversion of food into various forms of energy has brought Lipmann worldwide acclaim. His research will be continued in new laboratories to be constructed for him at the Rockefeller Institute, where he will also participate in the program of graduate education that was begun a year ago.

ROBERT F. THORNE of the State University of Iowa, and ROBERT L. WILBUR of North Carolina State College, share this year's George R. Cooley award of the American Society of Plant Taxonomists. They received the $500 prize for their outstanding papers published during 1955 on the flora of southeastern United States. Thorne's paper, which appeared in the *American Midland Naturalist,* is entitled "The vascular plants of southwestern Georgia." Wilbur's paper, published in *Rhodora,* is entitled "A revision of the North American genus *Sabatia* (*Gentianaceae*)."

The Cooley award of $100 for the outstanding paper presented before the society at its annual meetings went to HENRY J. THOMPSON, of the University of California at Los Angeles. Thompson's paper was entitled "A genetic approach to the taxonomy of *Mentzelia lindleyi.*"

Four awards for outstanding services to forestry were made at the 56th annual meeting of the Society of American Foresters.

SAMUEL T. DANA of Ann Arbor, Mich., received the Sir William Schlich memorial medal for distinguished services to forestry. Dana, a past president of the society and a former editor of the *Journal of Forestry,* is dean emeritus of the University of Michigan School of Natural Resources.

INMAN F. ELDREDGE of New Orleans, La., received the Gifford Pinchot medal, for outstanding service to forestry. Eldredge is a former member of the council of the society, a long-time officer of the U.S. Forest Service, and a well-known consulting forester. Until his retirement from the Forest Service he was director of the Forest Survey of the South.

LEO A. ISAAC of the Pacific Northwest Forest and Range Experiment Station, Portland, Ore., and PHILIP C. WAKELEY of the Southern Forest Experiment Station at New Orleans, each received two special awards for outstanding achievement in biological research contributing to the advancement of forestry.

TIBOR BENEDEK, associate clinical professor of dermatology, Stritch School of Medicine, Loyola University, has been appointed editor-in-chief of *Mycopathologia et Mycologia Applicata,* an international journal for pure and applied mycological research.

HARRY D. BRUNER, chairman of the department of physiology at Emory University, has been appointed chief of the Medical Branch of the Atomic Energy Commission's Division of Biology and Medicine. He succeeds ROY E. ALBERT, who has accepted a research and academic appointment at George Washington University (Washington, D.C.)

Recent Deaths

HUGH CHRISTISON, Methuen, Mass.; 72; retired chief chemist at Arlington Mills; 5 Nov.

WINIFRED C. CULLIS, London, England; 81; professor emeritus of physiology at the London School of Medicine for Women; 13 Nov.

HENRY A. HAUGH, JR., New Haven, Conn.; 59; electrical engineer; 14 Nov.

JAMES KNIVETON, Wyncote, Pa.; 58; heating engineer and vice president of Sales Corporation of America; 12 Nov.

EWING C. McBEATH, New York, N.Y.; 72; professor emeritus of dentistry at Columbia School of Oral and Dental Surgery; 13 Nov.

BENJAMIN SALZER, New York, N.Y.; 76; retired associate professor of neurology at the College of Physicians and Surgeons, Columbia University; 12 Nov.

HARRY S. TSCHOPIK, JR., Pleasantville, N.Y.; 41; assistant curator of ethnology of the American Museum of Natural History; 12 Nov.

Education

■ The Soviet Government has adopted new regulations for advanced scientific education, apparently with the objective of improving the training of persons receiving the Soviet degrees of Candidate and Doctor of Science, equivalent in the United States to the degrees of Master of Arts or Science and Doctor of Philosophy. The new decree was adopted by the Soviet Government and the Central Committee of the Communist party last August, but it was not publicized in the Soviet press until some time later. Its regulations replace rules governing graduate study that had been in force since 1937.

The most radical change has been made with respect to the degree of Doctor of Science. This will now be awarded only to persons making a significant contribution to knowledge. The 2-year period of study and research for the doctorate formerly customary in the Soviet Union has been abolished. Instead, persons seeking the doctorate are now expected to write their dissertations while they work in teaching or research posts.

Two major innovations have been made in the requirements for the Candidate of Science degree. Except for a few fields, such as mathematics and theoretical physics, no person will be accepted as a graduate student unless he has had at least 2 years of practical experience. In the past, most graduate students have been persons who had just finished their undergraduate training.

In addition the requirement of a dissertation for the Candidate of Science degree has been abolished and the new requirements demand only passing of examinations. However, persons writing a candidate's dissertation will be given preference in assignment of teaching and research positions.

The number of Soviet institutions permitted to grant doctors' or candidates' degrees has been sharply curtailed, particularly with respect to the higher degree. To attract people who are already working in scientific fields to graduate study, stipends for graduate study have been raised to match previous incomes, with a maximum of 1000 rubles monthly.

To assure tightening of requirements on dissertations, a Central Certification Commission with 77 members has been set up to review all dissertations. The power of this commission has been assured by two new regulations.

1) Persons receiving a higher degree on the basis of a dissertation will no longer receive an automatic pay increase after their departments have accepted the dissertation. The pay increase is now to go into effect only after the Central Certification Commission has accepted the dissertation.

2) Any department that approves a dissertation that is later rejected by the Central Certification Commission will lose for 2 years thereafter the right to accept candidates who intend to defend dissertations.

■ High school seniors throughout the country have been invited to compete in the 16th annual Science Talent Search of the Westinghouse Educational Foundation. Winners of the contest will share $11,000 in Westinghouse science scholarships. A 5-day trip to Washington, D.C., for the Science Talent Institute is also awarded to the 40 finalists in the contest. In the past 15 years, 4500 high-school seniors have won scholarships and honorable mentions in the Science Talent Search.

Entrants must report on an original

science project and take an aptitude examination. Their schools then submit scholastic records and teachers' estimates of the entrants' ability. To be eligible, entries must be received in Washington *by midnight 27 Dec.* The program is administered for Westinghouse by Science Service, Washington, D.C.

Grants, Fellowships, and Awards

■ The Markle Foundation has announced that it will continue for the tenth year its program of 5-year grants for young medical school faculty members seeking careers in academic medicine. The fund's Grants for Scholars in Medical Science were first offered in 1948 to give academic security and financial help to selected teachers and investigators. During the 9 years of the program a total of $5,320,000 has been appropriated toward the support of 181 doctors in 69 medical schools in the United States and Canada.

John M. Russell, executive director and vice president, has reviewed the program in the annual report of the foundation that has just been released. All but four of the Scholars "are active in academic medicine and are doing their part to improve teaching and research in medical science." A few have been forced to supplement low academic salaries from sources outside their medical schools, chiefly from private practice. Twenty-three have become heads of departments in medical schools, one directs an important cancer research institute, two head research divisions in government laboratories, one is dean of a medical school, and five are assistant or associate deans. Twenty-five have the title of full professor, and 51 are associate professors. Twenty-three of the group hold both the Ph.D. and the M.D. degrees, 142 have M.D.'s, and 16 have Ph.D.'s.

Approximately two-thirds of the scholars have taught and conducted research in the clinical sciences (pediatrics, surgery, medicine, and so forth), and approximately one-third are teachers and investigators in the preclinical sciences (anatomy, bacteriology, physiology, and so forth). "A study of the research of both groups indicates, however, that the great majority of those listed as 'clinical' are really conducting investigations at the basic level, while many listed as 'preclinical' have contacts with patients and are working to some extent, at least, at the applied level."

Russell reports that for the second year in its history, the income of the foundation totaled more than $1 million. The largest allocation, $690,000, was appropriated for support of the 23 Scholars in Medical Science, at the rate of $6000 a year for 5 years.

Three other grants were made in the field of medical science. The sum of $75,000 was given to Northwestern University Medical School for a 3-year study of an integrated program between preprofessional and professional training in medicine, and $115,000 was appropriated to the National Research Council Division of Medical Sciences; of this, $15,000 is for a study to determine whether an adequate number of young medical men and other scientists are being trained in research techniques in the health sciences, and $100,000 is for support of the council's fellowship program during the study. The third grant of $25,000 was made to the Harvard Divinity School as a contribution toward matching funds to meet a provision in the will of Mrs. T. W. Lamont. The late Mr. Lamont was president of the foundation.

In the Laboratories

■ Two United States firms have been selected to design and manufacture Venezuela's first atomic reactor and to furnish consulting engineering services, according to Humberto Fernandez-Moran, director of Instituto Venezolano de Neurologia e Investigaciones Cerebrales. The General Electric Company will design and manufacture a research reactor rated at 3000 kilowatts of heat, while the General Nuclear Engineering Corporation of Dunedin, Fla., will act as consulting engineers for the project. Walter H. Zinn, former director of Argonne National Laboratory, is president of General Nuclear.

The 2-year old IVNIC is an autonomous government agency and is international in character and scope. More than $5 million has already been spent on its buildings and equipment, and when the new facilities are completed more than ten times that amount will have been spent.

■ Houston Technical Laboratories, instrumentation subsidiary of Texas Instruments Incorporated, has officially opened its new plant in Houston, Tex. The company manufactures seismic exploration equipment, including the Worden gravity meter.

■ Battelle Memorial Institute has announced completion of the nation's first privately owned Nuclear Research Center. The announcement followed the initial start-up of the institute's 1-million-watt reactor. The new machine is the largest pool-type reactor yet to be put in service in this country. It is designed exclusively for research purposes.

It uses solid uranium-235 fuel. The core, composed of fuel elements and boron control rods, is suspended 22 feet below the surface of a large pool of highly purified water. The water serves as the coolant and as a shield to protect personnel from radiation. It is expected that the reactor will be operated 24 hours a day, 6 days a week. It was designed by Battelle physicists and engineered and built by the American Machine and Foundry Company.

■ Baxter Laboratories, Inc., Morton Grove, Ill., celebrated its 25th anniversary on 19 Oct. The company has been a pioneer in the commercial preparation of intravenous solutions.

Miscellaneous

■ Interscience Publishers, Inc., 250 5 Ave., New York, has announced publication of a new journal, *Combustion and Flame,* which will be the official journal of the Combustion Institute (International). Sir Alfred Egerton, F.R.S., is general editor. The journal is designed to act as a vehicle for papers that are at present widely scattered in many journals, and as an international forum for the discussion of problems of common interest to those working in the field of combustion. The journal will be quarterly and will have 500 pages and four issues per volume. The subscription price is $16 a year, postage free.

■ The U.S. Civil Service Commission has announced a new examination for filling engineering and physical science positions in activities of the Potomac River Naval Command in and near Washington, D.C., and in the Engineer Center, U.S. Army, Fort Belvoir, Va. The entrance salaries range from $4480 to $11,610 a year.

■ A German firm, Deutscher Buch-Export und -Import GmbH, Leipzig C-1, Postfach 276, has announced that it has available German translations of current Soviet scientific and engineering publications.

Appearing in the December issue of *The Scientific Monthly* are the following articles: "Man as a link in complex machine systems," G. H. Mowbray; "Genetic principles in human populations," H. J. Muller; "Some peaceful uses of atomic energy," W. Kenneth Davis, Shields Warren, Walker L. Cisler; and "Gravity program of the U.S. National Committee for the IGY," George P. Woollard. The "Association Affairs" section includes a proposed change in the AAAS constitution, a report of the Traveling High-School Science Library Program, and some programs of the 1956 AAAS New York Meeting. Eleven books are reviewed. The index to volume 83 of *The Scientific Monthly* is also included.

Reports

Implantation of Normal Blood-Forming Tissue in Radiated Genetically Anemic Hosts

Moderate x-radiation (200 r, whole body) affects a particular type (WW^v) of genetically anemic mice (*1*) more than it does their normal (*ww*, *Ww*) littermates. The radiation reactions of littermate pairs of adult mice, isogenic except for their *W*-genotype, were compared, using animals from two different interstrain F_1 hybrid crosses (*2*). These two groups consisted of offspring from matings of *Ww* parents from strains C and K with W^vw parents from a stock maintained isogenic with C57BL/6 by backcrossing in each generation (now b.c. 30 to 32). Strains C and K are strains favoring longevity of *WW* lethals; they were developed by selection and inbreeding [now F_{21} and F_{17} (*3*)]. As adults, anemic WW^v mice from each of these crosses have an erythrocyte count of 6 to 7×10^6 RBC/mm³ and a hematocrit mean of approximately 39 percent, while normal littermates show an erythrocyte level of 11 to 12×10^6 RBC/mm³ and a hematocrit mean near 49 percent. The mean and range of erythrocyte volume is 41.0 mμ³ (36 to 44) for normals, 57.6 mμ³ (51 to 66) for anemic mice.

Ten normal mice (5 C, 5 K, Table 1, controls) showed below normal hematocrits 5 and 9 days after radiation but had regained essentially normal levels by the 18th day. Nine anemic mice (4 C, 5 K, Table 1, controls) showed continued reduction in hematocrit level at 5, 9, and 18 days after radiation. Greater depression was observed in anemics of the K group than in those of the C group. Between 9 and 18 days, three of five K-anemics and one of four C-anemics died. The surviving anemics in both groups approached normal anemic hematocrit level by the 27th or 45th day. Determinations of mean cell volume 45 to 160 days after radiation indicated that normals and anemics were each forming cells characteristic of their genotype.

Nine normals (5 C, 4 K) and 8 anemics (4 C, 4 K) (Table 1, isologous injected) were given the same treatment as the control group, followed within 1 hour by tail-vein injection of approximately 8×10^6 cells from hematopoietic liver of 15.5-day-old C57BL/6 (*ww*) embryos. In normals of this group, the hematocrit drop was less than in controls, a fact suggesting almost immediate functional or mitotic activity of these injected cells. In anemics, this activity was even more apparent. The differences in hematocrit mean between injected and control anemics at 5 and 9 days represent for the total erythron approximately 3×10^9 and 6×10^9 erythrocytes in the K group, and 2×10^9 and 9×10^9 in the C group. This is at least 10 times the number that would have resulted from these embryo hematopoietic cells in normal position in neonatal mice.

From 18 days onward another significant phenomenon appeared. Hematocrit levels and, where established, mean cell volumes indicated that the isologous injected anemics of both groups were forming cells characteristic of normals. This constitutes strong evidence that injected hematopoietic cells from normal embryos became implanted in the radiated WW^v anemic host, where they functioned autonomously according to their own (*ww*) genotype. The slightly below normal hematocrit levels and somewhat larger than normal mean cell volumes in these anemics are most probably the result of the continued proliferation of WW^v cells but could conceivably indicate an influence of the host on the *ww* cells.

Homologous cells from the liver of 15.5-day-old C3H embryos had no protective effect when they were injected in three irradiated K-WW^v hosts (Table 1, homologous injected). From 5 to 18 days, C-WW^v irradiated anemics injected with C3H cells showed hematocrit levels above those of the controls, but at 27 days there was no evidence of phenotypic alteration. These limited data suggest that under the conditions of this experiment only isologous implanted cells persist for prolonged periods. Special radiation sensitivity of the blood-forming system of the anemic host, apparent in the data on radiated controls, combined with normal antibody-producing capacity as revealed by ability to reject homografts, may make it possible for the first time to separate two aspects of the problem of protection against effects of radiation by implantation of hematopoietic tissue. In former cases, heavier doses of radiation have broken down both blood-forming and antibody-producing systems of the host, so that homologous and even heterologous cells can implant (*4*). It may be significant that in our experiment irradiated anemics implanted with isologous hematopoietic cells are maintaining stable, relatively high hematocrit levels after 160 days.

This functional evidence of bone-marrow chimaera formation is based on ge-

Table 1. Effects on normal and anemic mice of 200-r irradiation, with and without injection of cells from embryo hematopoietic liver. Controls, no injection; isologous injected, injected with 8×10^6 C57BL cells; homologous injected, injected with 8×10^6 C3H cells. Abbreviations and units: Ht., mean hematocrit percentage; MCV, mean cell volume (mμ³).

Time (day)	K × C57BL F_1 hybrid						C × C57BL F_1 hybrid					
	Control		Isologous injected		Homologous injected		Control		Isologous injected		Homologous injected	
	Ht.	MCV	Ht.	MCV	Ht.	MCV	Ht.	MCV	Ht.	MCV	Ht.	MCV
	Normal (ww, Ww)						*Normal (ww, Ww)*					
0	50.4		48.8		51.3	39.1	47.4	41.6	46.4	42.8	49.0	42.4
5	39.5		45.3		43.8		39.4		42.7		43.5	
9	42.8		45.1		44.7		42.2		44.7		44.1	
18	48.6		49.0		51.0		46.6		48.8		47.6	
27	48.4		50.8		49.1		46.2		48.2		46.2	
45	49.9	41.2	50.2	44.2	49.7	43.7	47.4	42.0	47.0	40.4		
75	49.0	42.7	49.0	42.3	50.2	41.4	46.4	41.5	48.1	41.8		
105	50.1	40.1	45.9	40.9	47.8	39.5	46.6	41.6	47.7	42.5		
160	49.2	39.9	50.5	41.4								
	Anemic (WW^v)						*Anemic (WW^v)*					
0	38.4		40.3		38.7	60.6	39.8	56.8	38.0	58.3	40.2	60.5
5	18.0		22.0		18.2		24.9		27.7		29.2	
9	10.6		18.2		13.9		14.6		26.1		16.8	
18	7.9		37.5		7.9		10.8		44.6		21.6	
	(3d) –				(2d) –		(1d)					
27	23.2		46.1		22.7		38.1		46.3		34.3	
45	33.5	64.9	48.1	44.2	35.7	59.6	30.4	58.8	44.9	44.1		
75	34.4	58.6	46.8 (1d)	42.6	35.5	61.1	30.7	54.3	44.3	46.2		
105	38.3	61.3	45.8	43.6	37.8	67.6	35.0	58.2	44.3	46.4		
160	36.3	57.5	46.7	43.7								

netically controlled autonomy in rate and type of hematopoiesis of normal *ww* embryo cells implanted into irradiated WW^v anemic hosts. The concept of cell implantation is also supported by other data, including differential behavior of isologous and homologous implants. These data corroborate completely the recent evidence (*4*) that blood-forming tissue can implant successfully in irradiated host animals. They further give evidence that, insofar as *W*-gene effects are concerned, such implanted tissue acts in blood-formation according to its original genotype rather than in conformation to correlative influences from the body of the host.

ELIZABETH S. RUSSELL
LOIS JEAN SMITH
FAY A. LAWSON
Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine

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27 August 1956

Preparation of Nucleic Acids from Ehrlich Ascites Tumor Cells

Biological studies in progress at this laboratory have made it desirable to obtain undegraded nucleic acids from Ehrlich ascites tumor cells. A previous paper (*1*) described the physical properties of deoxyribonucleic acid (DNA) isolated from the tumor cells by several different methods.

This is a preliminary report on the isolation of highly polymerized ribonucleic acid (RNA) from Ehrlich ascites tumor cells and a comparison of nucleic acids prepared directly from the cytoplasm or nuclei with those isolated from the nucleoproteins.

Deoxyribonucleic acid prepared directly from Ehrlich cell nuclei by the method of Kay, Simmons, and Dounce (*2*) is highly polymerized and compares favorably with other material reported in the literature (*2*). Isolation of DNA protein by the method of Mirsky and Pollister (*3*) using 1*M* NaCl followed by duponol extraction also yields highly polymerized material. In contrast, DNA extracted in the same manner from DNA protein prepared by the distilled water method of Stern and others (*4*), and Gadjusek (*5*) is a degraded polydisperse material with an intrinsic viscosity of 5.2 in 0.2*M* NaCl.

A similar observation was made with RNA preparations. Ribonucleic acid, extracted by the method described in a subsequent paragraph from the ribonucleoprotein fraction obtained by the method of Mirsky and Pollister (*3*), is polydisperse and of low molecular weight. That derived directly from a cytoplasmic extract is paucidisperse and contains highly polymerized material. It is doubtful that the physical-chemical methods used in isolating the nucleoproteins are responsible for the depolymerization of the nucleic acids. Enzymatic degradation is a more reasonable explanation and has been discussed by Chargaff (*6*) and by Magasanik (*7*). In the case of Ehrlich ascites tumor cells, enzymatic degradation may be a more important factor in the isolation of RNA than of DNA, for the cells appear to be almost completely devoid of DNAse activity. Attempts to isolate highly polymerized RNA from Ehrlich cells employing duponal (*8*) or guanidine hydrochloride (*9*) have failed.

By the following procedure, RNA of high molecular weight was obtained. Ascites cells were frozen rapidly in a mortar immersed in Dry Ice-alcohol, then ground to a fine powder. The powder was homogenized in 0.14*M* NaCl containing 0.01*M* sodium citrate adjusted to *p*H 7. The nuclei were removed by centrifugation at 3000 rev/min for 20 minutes, and the supernatant was filtered. Ribonucleic acid was derived from this cytoplasmic fraction by extraction with phenol by the method of Gierer and Schramm (*10*). The RNA thus obtained was highly reproducible. Identical preparations were made from several batches of cells, both those that had been freshly harvested and those that had been held for several weeks at a temperature of −70°C. Yields were of the order of 1.0 to 1.5 g of RNA per 100 g wet weight of cells.

The RNA had a N/P ratio of 1.6 to 1.7. All the phosphorus could be accounted for as ribonucleotide phosphorus. The biuret reaction for protein was negative with samples as large as 40 mg, indicating that if protein is present, its concentration must be less than 0.5 percent. In addition, acid hydrolysis and partition paper chromatography for amino acids failed to show detectable contamination by protein. In 0.14*M* NaCl buffered with 0.01*M* citrate at *p*H 7.0, the material traveled as one peak in the electrophoresis apparatus. In the ultracentrifuge, there were four fast-moving peaks, and about 25 percent of the preparation, of low molecular weight, did not move away from the meniscus.

Table 1. Apparent relative concentration of the several components observed in the ultracentrifuge.

$S_{20°,H_2O}$	Relative concentration (%)
16	30
18	10
21	10
24	50

The RNA could be precipitated with ethanol. Drying with acetone appeared to cause partial denaturation and loss of solubility. The material was soluble in distilled water. The components of high molecular weight were slowly but more or less completely precipitated if the solution was made 1*M* with respect to NaCl. The material of low molecular weight remained in the supernatant. The RNA was stable for at least 72 hours at 4°C. The measured sedimentation constants dropped markedly in 10 days.

A sample of high-molecular-weight RNA was prepared for physical studies by precipitation with 1*M* NaCl. Its concentration was estimated by nitrogen analysis, assuming that RNA contains 14 percent nitrogen. Table 1 lists the apparent relative concentration of the several components observed in the ultracentrifuge when an RNA solution of 0.4-percent concentration was employed. The dependence of the sedimentation constants on concentration was small, and the measured intrinsic viscosity was 0.35. The ultraviolet absorption spectrum is shown in Fig. 1. The absorption maximum was at 257 mμ with $E_1^{1\%}{}_{cm} = 210$.

The high-molecular-weight RNA appears to be a compact molecule and is quite different from DNA. One is

Fig. 1. Ultraviolet absorption spectrum.

tempted to speculate that the chemical structure of highly polymerized RNA is perhaps different from that of DNA and contains cross links at the 2-position of the ribose. A purified sample of the $S = 24$ component should be very useful for fundamental studies of the structure of RNA. Attempts are being made to obtain homogeneous material (*11*).

JOHN S. COLTER
RAYMOND A. BROWN
Viral and Rickettsial Section, Research Division, American Cyanamid Co., Lederle Laboratories, Pearl River, New York

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Age, Growth, and the LD_{50} of X-rays

In the case of x-rays and other ionizing radiations, the dependence of the acute LD_{50} on age has not been determined throughout the life-span of a mammal, although it is known that immature mice (*1*) and rats (*2*) are more sensitive than mature ones, and it has been indicated that mature female rats are more sensitive at 16 months than at 6 months (*3*). The present experiments contribute data for the first 75 to 80 percent of the ~900-day life-span of CAF_1 mice.

The biological, physical, and statistical methods employed are described elsewhere (*4*). The mice were weaned at about 30 days and experienced puberty at about 50 days. The radiation factors were 250-kvcp x-rays; half-value layer, 1.6 mm Cu; tissue-dose rate, ~35 r/min. Deaths were counted during the 28-day period after a single whole-body exposure. For each LD_{50} determination, there were 4 to 6 dose-groups of about 10 animals each, thus employing a total of about 50 animals.

The results (Fig. 1) showed that the LD_{50} was a linear function of log age, A, from 37 to 105 days (*5*) but was practically constant from 115 to 709 days (*6*). Thus the hypothesis that the "adult" LD_{50} is proportional to remaining life-expectancy (*7*) was not confirmed.

A change occurred in the response of the oldest animals, shown by the increased value of S, the average of the LD_{84}/LD_{50} and LD_{50}/LD_{16} ratios. For ages 600 to 709 days, three determinations of S were 1.16, 1.17, and 1.24. For ages 37 to 434 days, the mean and standard deviation of 29 determinations of S were 1.072 ± 0.024. Radiobiologically, therefore, the population became more variable at 600 days.

The diphasic dependence of the LD_{50} on log age (Fig. 1) was similar to that for log body weight (*8*), a parameter of growth. It may be said, therefore, that for the first 80 percent of the life-span the LD_{50} continued to increase until the growth rate fell to its fully mature level, after which it remained constant.

HENRY I. KOHN
ROBERT F. KALLMAN
AEC Radiological Laboratory, University of California School of Medicine, San Francisco

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6. $LD_{50} = 721 - 23\ A \pm 32$; $\hat{\sigma}$ (23) = 31.
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8. Let $W = \log$ weight (g). For males, $W(37–103$ days$) = 0.43 + 0.50\ A \pm 0.02$; $W(115–709$ days$) = 1.30 + 0.083\ A \pm 0.02$. For females, $W(37–103$ days$) = 0.54 + 0.41\ A \pm 0.02$; $W(115–600$ days$) = 1.08 + 0.14\ A \pm 0.02$.

27 September 1956

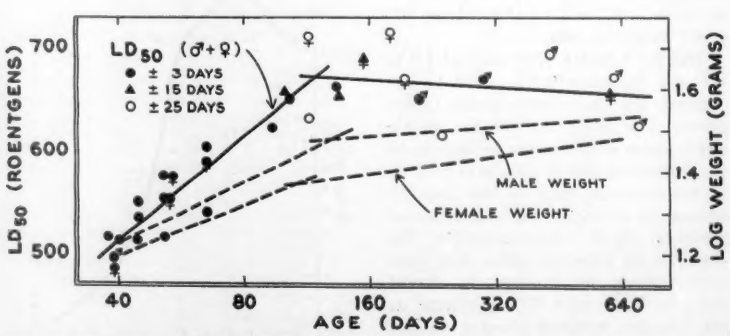

Fig. 1. LD_{50} and age (solid lines). Points without tails are for both sexes. The range in age for each point is indicated by the symbol. The dashed lines relate log body weight to age for the animals of these experiments.

Fowl Pox Vaccine Associated with Parthenogenesis in Chicken and Turkey Eggs

Studies were initiated at the Agricultural Research Center, Beltsville, Maryland, in 1953 to determine which factor or factors were responsible for the parthenogenetic development encountered in the eggs of chickens (*1*) and turkeys (*2*). Since these studies were initiated more than 13,000 infertile chicken eggs have been incubated and examined for parthenogenesis. These eggs were produced by chickens of four different breeds—namely; New Hampshires, Barred Rocks, Rhode Island Reds, and Dark Cornish—as well as two crosses involving New Hampshires and Dark Cornish. Data obtained showed an inherited tendency on the part of certain individuals, as well as on the part of different breeds, to produce eggs that will develop parthenogenetically. The eggs of Dark Cornish and Cornish crosses were the only ones encountered showing any appreciable amount of parthenogenesis that could be detected macroscopically. The development in the case of chicken eggs consisted typically of a limited growth of membranes. These data also indicate that this tendency can be increased or decreased by selective breeding. The causal agent remains unknown. However, it was found in 1955 that a virus may be involved in the initiation of parthenogenetic development in the eggs of Dark Cornish chickens.

In 1955, the eggs laid by 42 pullets from 19 January to 4 May were incubated and examined for parthenogenesis. On 5 May, these same 42 pullets were vaccinated, 13 with pigeon pox and 29 with fowl pox vaccine. All eggs laid by these 42 females during the following 4 months were likewise examined for parthenogenesis. In each case the method of testing was the same, an initial 9- to 10-day period of incubation, following which the eggs were broken and the germinal discs were examined macroscopically for parthenogenetic development.

In 1956, the eggs of 35 Dark Cornish females were examined before and fol-

lowing vaccination with fowl pox virus on 1 April. The results of the 1955 and 1956 tests are presented in Table 1.

These data reveal that, in 1955, more than 3 times as much parthenogenesis was encountered in eggs laid after vaccination with chicken pox virus than was found in eggs laid by the same birds prior to this time ($\chi^2 = 33.0$, degrees of freedom = 1, $P < 0.001$). It is interesting to note that the milder pigeon pox virus was less effective in inducing parthenogenesis ($\chi^2 = < 1$, degrees of freedom = 1, $P < 0.35$). In the 1956 tests involving fowl pox vaccine, the difference was even greater, more than 9 times as much parthenogenesis appearing in eggs of the same birds following vaccination than was found prior to the introduction of the virus ($\chi^2 = 168$, degrees of freedom = 1, $P < 0.001$). The highest incidence of parthenogenesis was encountered in eggs laid 30 to 60 days after the birds had been vaccinated. Facilities were not available for the maintenance of nonvaccinated birds during the full term of these tests. However, data on the incidence of parthenogenesis in turkeys does not show any appreciable seasonable variation (*3*).

A further indication that fowl pox vaccine may be involved in the initiation of parthenogenesis is furnished by data obtained in 1956 with turkey eggs. In these tests a total of 3110 eggs laid by two groups of turkeys were examined for parthenogenesis over a period of 3 months.

One group was composed of 16 nonvaccinated birds, the other of 49 turkeys that had been vaccinated for fowl pox at 7 weeks and again at 30 weeks of age. Since the ancestry of each bird was known, it was possible to select birds for these tests so that in each group full sisters would be represented. The 16 nonvaccinated turkeys, representing 12 families, were housed in wire cages within a screened building where they could be kept isolated from other birds. The 49 vaccinated birds, representing the same 12 families, were kept in turkey houses where they were in direct contact with other vaccinated birds. All birds involved in these tests were virgins, having been segregated from males at 4 weeks of age, and all received the same type of feed.

A total of 738 eggs was produced by the 16 nonvaccinated turkeys during the 3-month test period. Of these, 180, or 24.4 percent, showed parthenogenetic development when they were examined following a 9- to 10-day period of incubation. Membranes only were found in 144; 47 showed blood formation in addition to membranes; and 19 eggs contained well-formed embryos.

The 49 vaccinated females produced a total of 2362 eggs during the same period. Parthenogenetic development occurred in 750, or 31.8 percent, of these eggs.

Table 1. Incidence of parthenogenetic development found in eggs of Dark Cornish chickens before vaccination and following vaccination with pigeon pox and fowl pox vaccine.

Item	1955		1956
Before vaccination			
Number of birds on test	13	29	35
Number of eggs tested	497	1190	1294
Number of eggs showing parthenogenetic development	9	12	21
Percentage of eggs showing parthenogenetic development	1.8	1.0	1.62
After vaccination with	*Pigeon pox*	*Fowl pox*	*Fowl pox*
Number of birds on test	13	29	35
Number of eggs tested	808	1653	1675
Number of eggs showing parthenogenetic development	23	53	266
Percentage of eggs showing parthenogenetic development	2.9	3.2	15.9

The parthenogenetic development was classified as follows: 548 eggs contained membranes only; 100 contained blood in addition to membranes; and 102 contained well-formed embryos.

The records of these two groups of full sisters reveal that a significantly higher percentage of parthenogenesis occurred in the eggs laid by the vaccinated group of turkeys, 31.8 percent as compared with 24.4 percent of total eggs tested ($\chi^2 = 12.3$, degrees of freedom = 1, $P < 0.001$) (*4*). This increased incidence of parthenogenesis in the eggs of the vaccinated group was evident in each of the three categories listed—that is, membranes, blood formation, and embryos.

The results secured in the foregoing tests indicate that some agent, possibly of a physiological nature, had some part in initiating parthenogenetic development. When this agent was present at, or near, the optimum level in the blood stream of genetically susceptible hens, the development initiated tended to proceed further and show a higher degree of organization. The agent, whatever its nature, possibly may be transmitted from parent to offspring through the egg, since, in eggs of both nonvaccinated groups of chickens and turkeys, a considerable percentage of eggs was encountered showing parthenogenetic development. This was true, even though these nonvaccinated females were isolated from other birds, and every attempt had been made from time of hatching to keep them disease-free.

It would appear, therefore, that at least two conditions are necessary before an advanced type of parthenogenesis occurs. First, a susceptible strain of birds is necessary. This implies not only that birds produce readily activated eggs but also that the parthenogenetic cells possess a sufficiently high inherent viability to survive until such time as the eggs are placed in an incubator. In this sense the eggs produced by most of our domestic breeds of chickens cannot be considered as susceptible, since in nearly every instance the parthenogenetic cells have died by the time the eggs are laid. The second condition is that an activating agent must be present in the blood stream. When this agent is present at, or near, an optimum level, it has the effect of inducing parthenogenetic development.

It is not known, at present, whether the fowl pox virus as such is the sole agent initiating parthenogenesis or whether some contaminant which may be present in the vaccine is also involved. Neither is it known just how this virus may exert its effect, whether it is a direct or an indirect one.

M. W. Olsen
Agricultural Research Service, U.S. Department of Agriculture, Beltsville, Maryland

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4 September 1956

Spontaneous Activity in Denervated Insect Muscle

Following section and peripheral degeneration of the motor nerve supply, insect muscle generally has been thought to become inactive, even in response to direct electric stimulation (*1*). Recent physiological investigations of the thoracic spiracle muscles of cockroaches, *Periplaneta americana* (L.), have demonstrated a strikingly different mode of behavior, which is deemed of sufficient interest to the general question of the irritability of insect muscle to warrant preliminary description.

For each thoracic spiracle of the American roach there is a single occlusor

muscle which acts against the elasticity of the spiracle lips; hence, it remains continuously under tension. An experimental analysis of the innervation of these spiracles has demonstrated that they receive motor innervation from the median nerves (*2*). In more than 200 instances, median nerve section has prevented the spiracle from cooperating in the ventilative rhythm. Nevertheless, the spiracle muscle can remain contracted after complete denervation and will open in response to relatively high concentrations of CO_2. Spiracle movements for the first 3 to 5 days following denervation, although no longer coordinated with the ventilative pattern, appear otherwise normal; muscle twitches are rapid and involve the whole muscle. During subsequent days these contractions become slower and are produced by asynchronous activity of parts of the muscle. This behavior, which perhaps should be called fasciculation, continues unabated for at least 90 days or until the muscle is reinnervated by the regenerating median nerve.

At any time during this period the muscle may be removed to a dish of saline where it will continue to fasciculate. Surprisingly, during this entire period the muscle remains responsive to electric stimulation. Although the rheobase varies from 1.8 to 4.0 volts there appears to be no consistent variation with time since denervation. Electric stimuli do not interfere with the spontaneous fasciculative rhythm at low stimulus frequencies, but tetanic contractions are induced by liminal stimuli at about 20 per second.

A study of methylene blue preparations indicates that the onset of fasciculation occurs at about the same time that the peripheral branches of the sectioned median nerve disappear.

The differences between the behavior of denervated thoracic spiracle muscles and that described for other insect muscles are not yet understood and currently are under investigation. One possible source of the difference is that the spiracle muscle is continuously under tension. Consequently its mechanical arrangements are rather similar to those of some insect flight muscles, in which one set of muscles contracts against the tension of another, or to the dipteran haltere muscle, which contracts in opposition to the elasticity of the haltere articulation. In both the dipteran flight and the dipteran haltere muscles contraction is not necessarily the direct consequence of the motor impulse (*3*). Rather, the motor impulse serves to increase sensitivity of the muscle to stretch to such a degree that a twitch occurs. By analogy, it seems possible that fasciculation of the denervated spiracle muscle is due to increased sensitivity to stretch. This is supported by the observation that a denervated muscle which has become momentarily quiescent may be readily started fasciculating again by mechanically opening the spiracle. A corollary hypothesis that the presumed stretch sensitivity of denervation involves increased muscle sensitivity to the neuromuscular mediator substance is currently under investigation.

JAMES F. CASE

Physiology Division,
Chemical Warfare Laboratories,
Army Chemical Center, Maryland

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31 August 1956

Nicotinic and Glutamic Acids, Nicotinamide, and Glutamine in Cigarette Tobacco Smoke

During the course of studies of the constituents in tobacco smoke (*1*) it became of interest to investigate the presence of some naturally occurring, biologically active nitrogen compounds that had not previously been reported in the literature. The presence of the amino acids, glutamine and glutamic acid, and of nicotinic acid and nicotinamide are reported in this paper.

Cigarettes, 70 mm in length, were smoked according to a standard procedure reported elsewhere (*2*). An amphoteric fraction of the collected smoke containing nitrogen and giving a positive reaction to ninhydrin was chromatographed on Dowex-50 (8 percent cross-linked, 200 to 400 mesh, H^+ form), and six separate ninhydrin-positive fractions were eluted with 2*N* HCl. The fractions were compared directly with authentic samples of amino acids in different paper chromatographic solvent systems. Table 1 shows the close agreement of fracions 1 and 2 with glutamic acid and glutamine, respectively. After heating fraction 2 in 1*N* HCl for 1 hour at 100°C and rechromatographing, it was found that the spot which initially migrated exactly like authentic glutamine no longer existed. Instead, a new spot appeared which migrated parallel with known glutamic acid. The conversion after acid hydrolysis of fraction 1 into glutamic acid is additional evidence that fraction 1 was originally glutamine.

A quantitative determination that utilized the color developed by the reaction of the amino acids with ninhydrin showed that the smoke from a 50/50 blend of bright and burley tobaccos contained, respectively, 10 and 7 μg of glutamic acid and of glutamine per cigarette. The four remaining ninhydrin-positive amphoteric substances have not been identified.

The migration of the pyridine alkaloids and of nicotinic acid and nicotinamide in several solvents on buffered paper has been shown to be a function of the *p*H of the buffer (*3–5*). The pattern of movement of a given pyridine-containing compound in the system has been useful in its characterization (*4*). Nicotinic acid has been tentatively identified in tobacco smoke by use of such a procedure (*5*).

For a verification of the presence of nicotinic acid and a quantitative determination of the acid and of nicotinamide, the following procedure was followed. Smoke collected in Dry Ice-acetone traps was dissolved in ether and extracted three times with equal volumes of 2-percent HCl solution. The acid layer was adjusted to *p*H 10.0 with sodium hyroxide and extracted twice with an equal volume of ether to remove most of the nicotine-type alkaloids present. A check run with knowns showed that, after this treatment, all the nicotinic acid and 90 percent of the nicotinamide remained in the basic aqueous layer. This solution was then neutralized and concentrated to a small volume. An amount of solution corresponding to one cigarette was then applied in a 0.5-cm wide streak across the entire width of a dry, 20- by 40-cm Whatman No. 1 paper that had previously been soaked in *p*H 6.3 sodium citrate buffer.

On a separate 5- by 40-cm paper also buffered at *p*H 6.3, 20 μg of a known mixture of equal parts of acid and amide were applied and run simultaneously with the paper chromatogram containing the cigarette smoke. The R_f values of the acid and of the amide on the pilot paper were 0.30 and 0.80, respectively. The values were determined by developing a color reaction with *p*-aminobenzoic acid in an atmosphere of cyanogen bromide.

Two pencil lines that corresponded to the R_f of the acid and of the amide as

Table 1. Comparison of R_f values of glutamic acid and glutamine with elution fractions from Dowex-50 (H^+) ion-exchange resin.

Solvent system	Glutamic acid	Fraction 1	Glutamine	Fraction 2
Phenol/water (7/3)	0.27	0.27	0.56	0.56
n-Butanol/acetic acid/water (40/10/50)	0.22	0.22	0.12	0.13
Methyl ethyl ketone/propionic acid/water (75/25/30)	0.30	0.30	0.23	0.23

Table 2. Nicotinic acid and nicotinamide content of smoke from various tobaccos as determined by microbiological assay. The totals were determined by assay of the raw smoke without use of chromatographic separations.

Tobacco type	Micrograms per cigarette		
	Nicotinic acid	Nicotinamide	Total
Bright	13	6	14
Burley	15	7	21
Turkish	9	6	13

determined by the pilot run were made on the paper chromatogram of tobacco smoke. The areas that measured 2 cm above and 2 cm below the lines and running the width of the chromatogram were cut from the paper. These strips were eluted with 80-percent ethanol by the use of a Soxhlet extractor, and the eluted solution was concentrated to dryness. The solids were taken up in water and assayed by the standard microbiological method (*6*) using *Lactobacillus arabinosus* 8014 organism. Table 2 shows the results when this procedure is applied to smoke from cigarettes made from three different types of tobacco.

DONALD A. BUYSKE
JOHN M. FLOWERS, JR.
PELHAM WILDER, JR.
MARCUS E. HOBBS

Department of Chemistry,
Duke University,
Durham, North Carolina

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10 September 1956

Solubility and Color Characteristics of Leaf Proteins Prepared in Air or Nitrogen

A characteristic property of extracts made from plant leaves is the intense brown color that develops almost immediately upon disruption of the living leaf cells. The browning reaction, presumably arising from the action of polyphenoloxidase, is often so severe that it interferes greatly with the analysis of the leaf proteins by such methods as electrophoresis and analytic ultracentrifugation, which depend upon an optical system for the location of protein components. Aside from this difficulty, browning also indicates that an undesirable change has occurred from the natural state of living protoplasm.

Browning of leaf extracts has been partially controlled by the use of chemical inhibitors (*1*). However, in our experience, inhibitors such as cyanide and dithiocarbamate have the undesirable property of precipitating proteins. Also, the addition of chemicals to plant extracts further complicates an already complicated system. The purpose of this communication is to show that browning can be entirely prevented when all operations during the homogenization and fractionation of leaf extracts are conducted in an inert atmosphere of high-purity nitrogen. Under such conditions, there is a striking difference in the solubility and color properties of two proteins that we have investigated, fraction I protein (*2*) and tobacco mosaic virus (TMV). We are presenting a general outline of the method because of its routine adoption in this laboratory for our work on plant proteins and viruses (*3*).

Leaves are first infiltrated repeatedly in a vacuum desiccator with 99.99 percent nitrogen which has been further purified by passage through alkaline pyrogallol and water. The sealed desiccator is then placed in a specially constructed plastic box (*4*). The box (Fig. 1) is designed to accommodate apparatus such as an Omnimixer, basket centrifuge, Servall centrifuge, electrodes for *p*H measurements, and glassware. Access to the apparatus within the box is provided by the neoprene gloves attached to the arm holes in the front panel.

In order to prevent browning of leaf extracts, we have found that a box atmosphere of more than 99.5 percent nitrogen is necessary. To obtain this, the box is sealed and then flushed with purified N_2 for 1 hour at a flow rate of 15 lit/min. Air and excess N_2 escape through small outlets at the top of the box. Nitrogen is played over all surfaces of the interior and apparatus and is bubbled for a few minutes through all liquids by means of a sintered glass gas disperser. After the flushing operation, the nitrogen flow is decreased to 8 lit/min, and the outlet tubes at the top are clamped off almost completely in order to maintain a slight positive pressure in the box.

We shall present here only one of the anaerobic protocols that we have followed. The problem was to prepare an anaerobic tobacco leaf extract free of cell walls, nuclei, chloroplasts, mitochondria, and sphaerosomes and to compare the remaining soluble proteins with a similar aerobic preparation. For this purpose, half-leaves of tobacco were used, one set for the anaerobic preparation, the other for the aerobic preparation.

The desiccator was opened in the flushed work box, and the leaves were

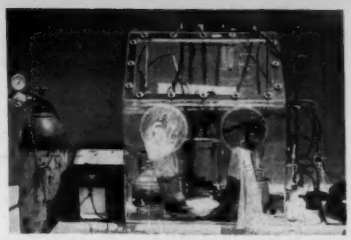

Fig. 1. Plastic box for maintaining an inert atmosphere during the preparation of leaf proteins.

cut into small pieces about 1 cm^2 in size. The leaf pieces were homogenized at top speed for 5 minutes in 0.5*M* *p*H 7.5 maleate buffer (1 part by volume to 2 parts of leaves by weight) in the Omnimixer, the Omnimixer cup and sleeve to which it was attached being immersed in an ice bath. The homogenate was passed through sharkskin filter paper with the aid of a basket centrifuge. The filtrate had a brighter green color than similar aerobic preparations.

The filtrate was placed in Spinco centrifuge tubes, the gaskets for the tubes were lightly greased, and the caps were tightened more than is customary in order to prevent entrance of air. The tubes were then removed from the box and centrifuged in the Model L Spinco ultracentrifuge, No. 30 head, at 25,000 rev/min for 90 minutes. The centrifuge tubes were returned to the nitrogen atmosphere of the work box before being opened. The clear, light yellow supernatants, designated whole cytoplasm, were set to dialyze under nitrogen against 0.1 μ maleate buffer at *p*H 7. They were then anaerobically transferred to a synthetic boundary cell for analytic centrifugation. The aerobic preparation of the opposite half-leaves resulted in a clear supernatant which was dark brown in color.

Analytic centrifuge patterns of both the air and the nitrogen samples appear in Fig. 2. The greater transmission of light through the anaerobic preparation permits the use of a narrower slit width, thus defining the baseline more sharply. The lesser difference in light transmission between the buffer and the solution makes for sharper definition of the forward limb of the slow moving peak. All in all, area measurements made with anaerobic preparations are more precise. The sedimentation constant of the faster component, representing fraction I protein, measured 17.8*s* for the aerobic whole cytoplasm and 18.1*s* for the anaerobic, a difference that is not considered significant.

By centrifuging the anaerobic whole cytoplasm, under anaerobic conditions as before, at 40,000 rev/min for 3 hours in the No. 40 Spinco head, clear, light yellow pellets, mostly consisting of frac-

tion I protein, are obtained. The aerobic counterparts of these pellets are clear but dark brown in color.

In addition to the striking difference in coloration of fraction I protein, a remarkable difference in its solubility characteristics is apparent. Fraction I protein pellets prepared in N_2 dissolve completely with great ease and rapidity merely by gentle shaking. Indeed, one difficulty in the anaerobic method is that the protein dissolves appreciably before the N_2 atmosphere can be reestablished and the supernatant fluid decanted. This difficulty can be partially overcome by not filling the tubes quite full and then immediately inverting the tubes after centrifugation, so that the pellets are separated from the supernatant liquid by a N_2 bubble during the interval necessary to reestablish an external N_2 atmosphere. When it is completely dissolved, a solution of fraction I protein is water clear, even when it contains as much as 1 percent protein. In contrast, fraction I protein prepared in air dissolves slowly and incompletely with an undissolved residue of protein remaining in spite of vigorous stirring. The protein solution is dark brown in color.

When the fraction I protein pellets are resuspended in weak neutral buffers or water, the anaerobically prepared pellets still require less time to resuspend completely than do the aerobic pellets. Thus, the same solubility relationships exist regardless of the resuspending medium. The following are some other observations comparing the behavior of the same leaf extracts prepared in air and under N_2.

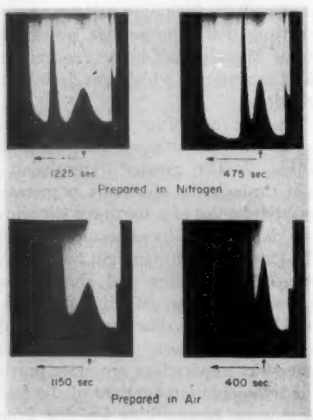

Fig. 2. Schlieren patterns of tobacco leaf proteins during centrifugation to illustrate clarity of pattern when browning is prevented. Arrows indicate direction of migration and position of starting boundary in synthetic boundary cell. Time is indicated after rotor attained speed of 51,100 rev/min; 0.1 μ K maleate buffer *p*H 7.0, protein concentration 0.8 percent. Fastest moving component is fraction I protein.

1) The ability to brown when exposed to air is retained for at least 3 weeks by anaerobic preparations of whole cytoplasm of tobacco leaves when the preparations are kept anaerobic at 4°C. With a similar preparation of spinach leaves, the ability of whole cytoplasm to brown is lost in 1 day.

2) Sixty hours of anaerobic dialysis (bubbling N_2) of anaerobic whole-leaf cytoplasm of Turkish tobacco, involving 10 anaerobic changes of a 1/50 dilution of the 0.5*M* maleate buffer, results in the loss of ability to brown in air. Presumably, a substrate or cofactor for the browning reaction has been lost during dialysis. On the other hand, *Xanthium* leaf whole cytoplasm will brown on exposure to air after a similar 60-hour anaerobic dialysis. Evidently, a less readily dialyzable component is contained in *Xanthium* extracts which is involved in oxidative browning.

3) Anaerobiosis permits the detection of pigments that are masked by browning. For example, tobacco leaf cytoplasmic proteins, dialyzed against phosphate buffer before analysis in a Cary recording spectrophotometer, display a minimum at 250 mμ, a maximum at 267 mμ, and shoulders at 290 and 325 mμ. The 325 mμ wavelength no doubt contributes to the yellow color of the protein solution. In contrast, a brown cytoplasmic protein solution fails to provide as much spectral detail in the ultraviolet and displays nothing more than a continuous absorption in the visible region of the spectrum.

4) During the summer and autumn months, a brownish purple pigment has been observed to occur in two species of tobacco. The pigment is partly dialyzable and is insoluble in all of the common organic solvents but is soluble in water. The cultural conditions of the plants under which the pigment appears are still undefined but may have some connection with nitrogen deficiency.

5) A red layer, lying between the starch and green chloroplast residue layers of the pellet formed by centrifuging a tobacco leaf homogenate at 30,000 rev/min (No. 30 Spinco head) for 1 hour, although observed to occur under aerobic conditions, is very much more pronounced under anaerobiosis.

6) Anaerobiosis permits the preparation of the common strain of tobacco mosaic virus in an uncolored condition, whereas TMV prepared in air under the same conditions is brown in color. As was shown previously (*5*), a brown host nucleoprotein associated with the virus can be removed by chelating agents to yield a colorless virus. Anaerobically prepared, uncolored virus also responds to the presence of a chelating agent by releasing, in this case, presumably an uncolored nucleoprotein. Anaerobically prepared virus also resuspends more rapidly than its aerobic counterpart.

As a result of our experience with the anaerobic method applied to a variety of plant physiological problems, we have come to believe that it represents a significant step forward in the direction of preparing protoplasmic constituents from plants which correspond more closely to their native state in the living cell. While the method requires some extra effort in the form of inconvenience, nevertheless the protoplasmic products obtained are of sufficient interest to warrant the inconvenience.

Morris Cohen*, W. Ginoza,
Robert W. Dorner,
William R. Hudson, S. G. Wildman
Department of Botany,
University of California, Los Angeles

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2. L. Eggman, S. J. Singer, S. G. Wildman, *J. Biol. Chem.* 205, 969 (1953).
3. Supported, in part, by research grant, RG-3939, National Institutes of Health, U.S. Public Health Service, contract AT(11-1)-34, project 8 of the division of biology and medicine, U.S. Atomic Energy Commission, and Cancer Research Funds of the University of California. We wish to acknowledge the close cooperation of Luther Eggman, Howard Boroughs, Ed Carusi, and Albert Kahn in this project.
4. Constructed by Hollywood Plastic Arts, Los Angeles, Calif.
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* Fellow of the American Cancer Society, 1953–54, as recommended by the Committee on Growth, National Research Council.

16 August 1956

Reserpine and the Learning of Fear

It has been reported (*1*) that reserpine will attenuate a conditioned "emotional" or "fear" response in rats and monkeys. This finding was confirmed by other investigators (*2*) who, in addition, reported that monkeys that had been drugged prior to conditioning were "functionally impervious" to acquiring the emotional response. In contrast, the present paper presents the findings of two experiments that indicate that reserpine, even in large dosages, has a surprisingly minor effect on the learning of a fear response in rats (*3*).

The method used to establish and measure emotional behavior has been described in detail elsewhere (*4*). Briefly, thirsty rats are trained to press a lever for a water reward on a variable-interval schedule—that is, a lever press produces the reward only occasionally (average, once in 60 seconds). When the response rates are stabilized, the conditioned emotional response is superimposed on the lever-pressing behavior by repeated presentations of an originally neutral stimulus, a 3-minute clicking noise, terminated

Table 1. Emotional conditioning measured as suppression of lever responding in the presence of the conditioned stimulus during an extinction test period. Values presented express the rate of lever pressing during the presentation of the 3-minute auditory stimulus as a proportion of the rate in the immediately preceding 3-minute period.

Experiment I			Experiment II		
Rat no.	First test trial	Over-all extinction (15 trials)	Rat no.	First test trial	Over-all extinction (15 trials)
		Reserpine during conditioning			
E1	0.00	0.56	E4	0.03	0.80
E2	0.02	0.59	E5	0.01	0.15
E3	0.00	0.48	E6	0.00	0.64
Mean	0.01	0.54	Mean	0.01	0.53
		Saline (or no drug) during conditioning			
C1	0.00	0.52	C4	0.00	0.54
C2	0.00	0.26	C5	0.00	0.46
C3	0.00	0.55	C6	0.00	0.14
Mean	0.00	0.44	Mean	0.00	0.38

contiguously with a brief shock to the feet.

The conditioned fear response consists of suppression of lever responding, crouching, immobility, and frequently urination and defecation on presentation of the auditory stimulus. A quantitative index of the degree of behavioral suppression is obtained by calculating the ratio of the number of lever-pressing responses emitted in the 3-minute period of clicker presentation to that emitted in the 3-minute period immediately preceding the onset of the stimulus.

The present experiments were similarly designed, and both involve two main phases. In the first phase, two groups of rats were given emotional conditioning; an experimental group was trained under the influence of reserpine, and a control group was given identical training without the drug. In the second phase, both groups were tested for emotional conditioning after the effects of the drug had worn off. This kind of experiment requires two controls: the experimental group must be effectively drugged during conditioning, and the effects of the drug must have been dissipated prior to testing.

The first experiment investigated the chronic action (that is, conditioning sessions occurred 23 hours after drug administration) of reserpine on the learning of the fear response. Following stabilization of lever-pressing rates in daily 1-hour sessions, three experimental animals were given intraperitoneal injections of 0.5 mg/kg of reserpine daily after the lever-pressing run, and three control rats received saline injections of equal volume. Injections were continued for 6 additional days, at the end of which time a sharp drop in lever-pressing rates (on the average, to approximately 60 percent of the original level) indicated that the drug had "taken." On each of the four days immediately following, three pairings of clicker and shock (1.0 ma) were administered at 20-minute intervals during the 1-hour lever-pressing session. Every animal thus received a total of 12 emotional conditioning trials. Drug injection was continued during this period.

Prior to testing for emotional conditioning, a control phase was instituted to take account of the long-term residual effects of reserpine. For 10 days immediately following the conditioning phase, the drugs were switched between groups; the control group received a series of reserpine injections identical with that administered to the experimental animals prior to and during conditioning, and the experimental animals were given saline injections corresponding to those previously received by the controls. During this period there were no emotional conditioning trials. Following this control phase, all injections were discontinued, and the daily lever-pressing sessions were continued until the original response rates had been recovered by all animals (24 days). Both groups then received five daily extinction sessions under a procedure in which the clicker stimuli were presented as in the conditioning phase, but no shocks were administered.

The second experiment, which was concerned with the acute (3 hours between injection and conditioning) action of reserpine, essentially replicated the procedure of the first experiment with the following changes (*5*). On the three days prior to emotional conditioning, experimental animals received "preparatory" injections of 0.5, 0.5, and 1.0 mg/kg of reserpine, in that order. Twelve emotional conditioning trials were then administered to both experimental and control (noninjected) animals over two daily sessions, the experimental animals having been injected with 1.0 mg/kg of reserpine 3 hours prior to the first conditioning trial of each session. Injections were then discontinued, and daily lever-pressing runs were administered until the original response rates had been recovered by the experimental subjects (10 days). All animals were then given five daily extinction test sessions identical in procedure with that of the first experiment.

An index of lever-pressing suppression was calculated as described for every animal in both experiments for the first test trial and for the over-all extinction period. These data are presented in Table 1. A high degree of emotional conditioning for all animals, experimental and control, is indicated by the first extinction trial scores; the values in all cases equal or approximate 0.00, reflecting virtually complete suppression of lever responding during the presentation of the clicking noise. Further, examination of the over-all test period scores reveals only a slightly greater tendency on the part of the controls to resist extinction of the conditioned suppression.

In view of the fact that the experimental animals were drugged practically to the point of inactivity by the end of the conditioning phase (*6*), these data lend little support to the earlier report (*2*) that reserpine substantially impedes the learning of an emotional response. While this discrepancy may be a species phenomenon (rats versus monkeys), the similarity of reserpine effects on previously established emotional behavior in these animals (*1*) suggests that procedural differences are more likely factors. The use of a relatively objective criterion of emotional response in the present studies, as opposed to the observational criteria employed in the earlier investigation, is one difference in method that has, in other situations, not infrequently yielded conflicting findings.

LARRY STEIN

Walter Reed Army Institute of Research, Washington, D.C.

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5. More accurately, this experiment investigated both cumulative and acute effects of reserpine.
6. For this reason, reliable estimates of suppression were not obtainable in the conditioning phase. However, bolli counts taken during conditioning in experiment I suggest that the experimental subjects were less fearful. The total counts for the three drugged anmials were 3, 11, and 14, while those for the controls were 32, 25, and 35.

20 September 1956.

Book Reviews

The History of the British Flora. A factual basis for phytogeography. H. Godwin. Cambridge University Press, London, 1956. 384 pp. Illus. + plates. $16.50.

The flora of the British Isles has long been studied, and there have been many interpretations of its origin, development, and present distribution. The book under consideration represents the most recent and, by all counts, the most comprehensive attempt to interpret this interesting flora on a strictly factual, empirical basis. It is the outcome of more than 30 years' study of Quaternary history by the author, who is a fellow of Clarke College, Cambridge, and university reader in Quaternary research. Indeed, every botanist is familiar, at least in a general way, with the writings of Godwin, especially on peat and its included pollen.

Here, for the first time, all available references to the occurrence of flowering plants in the British Isles through the Quaternary to the Recent have been brought together and critically evaluated. There are discussions of the collection and identification of plant remains (which include, of course, many things other than pollen), of the subdivision of the Quaternary and the various means of dating these divisions, and finally of the sites of collection. The body of the book, some 220 pages, comprises the detailed presentation of the actual plant record. This is, necessarily, not something for light reading. But throughout this section are interspersed general discussions of the fossil remains of particular families, genera and species, and these are not only interesting reading, but they often have general application beyond the scope of the British flora.

The most fascinating part of the work is the concluding sections analyzing the pattern of change in the flora and the general conclusions which may be drawn from the record. The arrival and spread of species is documented and correlated with climatic and geologic changes. In great detail are described the progression from a flora which contained many arctic-alpine species (in the time of the ice), the rapid invasion across the then dry North Sea of a large proportion of the present flora which contained many thermophiles, the development of the sea barrier and the effects of early man. Clearing the existing vegetation (thus opening habitats to migrules), introducing fire, and deliberately cultivating plants are among the effects of prehistoric husbandry which are clearly recorded by the fossils. Interesting comparisons between Ireland and Great Britain also are made.

I found *The History of the British Flora* most interesting and am sure that it will be one of the important and often-used references on my shelves.

RICHARD W. HOLM
Stanford University

Color Atlas of Oral Pathology. Histology and embryology, developmental disturbances, diseases of the teeth and supporting structures, diseases of the oral mucosa and jaws, neoplasms. U.S. Naval Dental School, National Naval Medical Center. Lippincott, Philadelphia, 1956. 188 pp. Illus. $12.

In the foreword and preface R. W. Malone, B. W. Hogan, and Robert A. Colby state that the atlas was prepared primarily for use in the officer training programs of the Naval Dental Corps and for general practitioners. As is stated by Colby in the preface, the book is strictly an atlas, and all pictures are in color except the x-ray copies. The text is printed on excellent paper, and the photographs are beautifully reproduced. The photomicrographs, clinical photographs or x-ray copies, and the legends are always on the same page.

The book has five divisions, namely: histology and embryology; developmental disturbances; diseases of the teeth and supporting structures; diseases of the oral mucosa and jaws; and neoplasms. The first section, on histology and embryology (15 pages), covers the histology of the skin, mucous membranes, jaw structures, tongue and bone. It has some excellent photographs and figures of developing human embryos. The second section (32 pages) deals with disturbances such as cleft lip and palate, various hypertrophies and atrophies, cysts, tori, Fordyce's disease, and various disturbances in tooth and bone development.

Although on page 42 there is the statement (as there is in nearly all textbooks on the subject) that an excessive amount of fluorine in the drinking water is the most frequent systemic factor in enamel hypocalcification, this has never been conclusively demonstrated.

The concept of hypocalcification most likely arose from the microscopic appearance of ground sections of fluorosed teeth. The affected areas alter the transmission of light. Also, fluorosis is the only enamel defect for which the specific etiological factor has been determined, but the relative frequency of occurrence in comparison with other types of defects is not known, and the effect of fluoride on the calcium content of enamel has not been clearly established.

The third section (36 pages) describes such lesions as tooth abrasions and erosions, stains, root fractures and resorptions, cementicles, dental caries, diseases of the pulp and periapical structures, various types of stomatitis and gingivitis, periodontal diseases and hyperplasias. On page 78, in commenting on periodontitis, appears the statement: "At first the inflammation may be aseptic, but calculus (which is always found in a peridontal pocket) irritates the crevicular epithelium, causing minute ulcerations that provide an avenue for the invasion of microorganisms and the absorption of toxic material." The reader should be cautioned not to interpret this as meaning that the early lesion of periodontitis may be sterile. No bacteriologist would conclude that any mouth is sterile, whether or not there is clinical evidence of inflammation. It is common knowledge among dentists that gingival tissues appear much healthier after a prophylaxis, owing to removal of microorganisms, calculus, debris, and so forth. Sections of periodontal tissues contain microorganisms and always demonstrate either acute or chronic inflammation. Inflammation is known to occur only after tissue injury. Microorganisms found in the mouth are known to be capable of inciting inflammation and are found at the site of the inflammatory process. In addition, calculus is known to contain myriads of microorganisms. It must be emphasized that overwhelming evidence indicates gingival and periodontal lesions are infected.

The fourth section (43 pages) considers lesions that result from soft tissue injury, chemical and thermal burns, radiation effects, drug idiosyncrasies, keratotic lesions, retention cysts, sialolithiasis, herpetic lesions, tuberculosis, syphilis, histoplasmosis, actinomycosis, lipoid storage diseases, vitamin deficiencies, and lesions resulting from hormone imbalances.

The fifth section (31 pages) is an excellent presentation of practically all oral neoplasms. The remaining 20-page section is a bibliography.

Except for the somewhat controversial aforementioned points, the atlas is excellent. It will be particularly helpful to general practitioners of dentistry and medicine, whereby at a glance in most cases, a clinical photograph, a photomicrograph, an x-ray, and a brief description of a lesion may be obtained. It will also prove valuable and is highly recommended to graduate students and residents in oral and general pathology as well as to the practicing general pathologist who may not see many oral pathological lesions.

HAROLD M. FULLMER
National Institute of Dental Research, National Institutes of Health

Proceedings of the International Conference on the Peaceful Uses of Atomic Energy. vol. 8, *Production Technology of the Materials Used for Nuclear Energy*. United Nations, New York. 1956. 627 pp. Illus. $10.

This large volume is the eighth in the series of 16 books that record the great unveiling of classified material that took place at Geneva in 1955. It contains 95 papers in four major sections: treatment of uranium and thorium ores; production of metallic uranium and thorium; analytical methods in raw material production; production technology of special materials. The last section is devoted to heavy water, graphite, zirconium, and beryllium.

Developments in the basic sciences underlying nuclear power production have been published quite freely, but information on technologic processes has been severely restricted. Hence, this book is of great interest for its content of technical material alone. In addition, however, one is naturally curious about the relative contributions of different nations to this massive international declassification. Curiosity centers particularly upon a comparison of the information furnished by the United States and the U.S.S.R. I satisfied my wonder by compiling a few numbers that are offered without intent to draw conclusions. In the category of technology, the United States contributed 18 papers for a total of 195 pages, while the U.S.S.R. delivered four papers totaling 16 pages. Pursuing the counting in all four sections of the book a bit further, one may be somewhat astonished to find 12 papers from Yugoslavia and 15 from Argentina but only three from the United Kingdom and one from Germany. As might be expected, countries that do not yet have a full nuclear energy program have concentrated their efforts on work that can be performed with small quantities of material.

A second intriguing question bears on the value of secrecy as a means of achieving a lead over other nations. Given the same goals under conditions of isolation, will scientific workers reach similar solutions? Judging from the few examples in this book where comparison is possible, the answer seems to be affirmative. Thus, the methods of producing beryllium in this country and in the U.S.S.R. are strikingly similar. The same is true of the manufacture of metallic uranium. In this case, all four nations reporting use a bomb reduction of uranium fluoride with an alkaline earth metal, although three employ calcium, while the United States prefers magnesium. On the other hand, the greatest diversity appears in the field of analytic chemistry where security restrictions have been very mild.

The quality of most of the papers is excellent, and many contain a wealth of detail. This is true of those describing manufacturing methods as well as those on analysis. As a result, this book will repay careful study by all who are concerned with the production technology of the basic constituents of nuclear reactors. It is to be hoped that the 1955 Geneva conference is the first of many similar international meetings.

M. KOLODNEY
City College of New York

Principles of Embryology. C. H. Waddington. Macmillan, New York, 1956. 510 pp. Illus. $7.50.

C. H. Waddington's new book is intended as a textbook for advanced university students (alas, he does not expect them to be able to read German): a review of recent work on the analytic or causal aspects of developmental science. All embryologists will, it is safe to say, derive both pleasure and stimulation from its perusal: pleasure because of the easy mastery with which experimental results from dozens of different laboratories on several continents have been woven into a coherent exposition, and stimulation because, of course, nobody will agree completely with the choice of material or with every interpretation offered. The figures and diagrams are particularly well chosen insofar as they are selected from the literature, and the original ones are very neat and illuminating. One may cite numerous examples of particularly felicitous exposition which confer on sometimes difficult subjects a deceptive air of simplicity and clarity.

The book is subdivided into two major parts. The first, called "The facts of development," has chapters reviewing the experimental analysis of successive stages of life-history and separate chapters on the major animal groups that have been objects of experimentation. The section ends with chapters on growth and regeneration. It is perhaps a reflection of the state of the analysis that the chapters dealing with early phases of development appear much more successful than those on organ development and regeneration. The second part of the book is concerned with "Fundamental mechanisms of development," particularly genetic and biochemical mechanisms. It is very good to see the data of developmental genetics given equal prominence with those of experimental morphology between the two covers of a single book; this proximity, however, seems to demonstrate how far apart these two disciplines—which should by rights be one discipline—are in *Fragestellung* and emphasis, even when they use the same or similar methods. Perhaps in his next book Waddington will be able to remedy this.

In a textbook of this comprehensive scope it can scarcely be expected that every aspect of the material has been treated with equal breadth and felicity or that a strict account of priority in experimentation has been rendered. The author absolves himself from the latter obligation so neatly in his preface that it would be hard for even the most obsessive critic to object. A sense of the history of a discipline is not perhaps essential to analytic attack: Waddington writes as a practical mechanist, physiographer and topologist, and morphogeneticist. It would be ungrateful to ask for more. However, I confess to being somewhat baffled by some of the definitions in the introductory chapter—unconvinced, for example, of the reality of the distinction between "field of competence" and "individuation field." Indeed, the later chapters on the embryonic axis and "individuation" are so reasonable and readable that we strongly suspect the author to have been guilty of writing his first chapter before, rather than after, the last: a warning to all of us.

DOROTHEA RUDNICK
Yale University

World Sea Fisheries. Robert Morgan. Pitman, New York, 1956. 307 pp. Illus. $6.95.

Why do some oceanic islands import a large portion of the fish their inhabitants consume? What is the influence of a coastline on the development of local fishing industries? These are the types of problems that this book tries to analyze.

Commercial fish harvest, like the exploitation of any natural resource, is influenced by many factors, and economic and geographic ones are often more important than the biological potential of the region in question. This is one of the main tenets of the book, and the author stresses it throughout.

There are three sections; the first deals with the physical environment, briefly tells about marine productivity, and surveys the potentials of fish harvest the world over. A chapter on the economically important types of sea fishes is also included here.

The second section treats fishing gear and techniques of fish distribution and storage. Fishing ports and communities are classified, leading into the third section, which is an analysis of world fisheries by regions. Stated criteria of comparison are the stages of technical development, total and per capita production of fish, imports and exports, to name the most essential ones. Despite his stating of these criteria, the author deals essentially with continents or parts of continents as units of discussion. Asia, Africa, Europe, North America, Central and South America, and Australasia are the chapter headings. A short chapter on whaling and one on the changing aspects of fisheries terminate the book.

It is not easy reading, because a wealth of information has been compressed into relatively few pages, and, in spite of a somewhat textbook like style and organization, the reader is often left with abstracting some general principles from the individual chapters.

In certain places one might have liked to see more stress on the historical development of fisheries, and the final chapter especially would have benefited by considerable expansion with a more discoursive summing up than has been given on the relative role of fresh-water and marine fisheries in the food economics of the future.

As a survey of world fisheries, the book is up to date to 1953 and it does fulfill its publisher's claim—that of being comprehensive. Thus it supplements earlier books of this kind, such as Tressler and Lemon's *Marine Products of Commerce*, still outstanding, however, because of its detailed treatment of fisheries technology and information on all kinds of marine products apart from fish.

JOHN E. BARDACH
University of Michigan

Jurassic Geology of the World. William J. Arkell. Hafner, New York, 1956. xv + 806 pp. Illus. $16.50.

William Arkell has been studying the Jurassic and ammonities for a number of years. This impressive work is "the first attempt at a synthesis of one system on the basis of marine faunas in all parts of the world."

The book is divided into eight parts: introduction; western and southern Europe; Africa and Arabia; southern Asia; Australia; north-eastern Asia; America and the Antarctic; and a general survey and conclusions.

Arkell considers the ammonites the best guide fossils of the Jurassic because of their short vertical range, wide horizontal distribution and, in part, their ease of recognition, whereas the pelecypods and gastropods often do not have the first two of these requirements. In nonammonite beds other fossils must be used and, in fresh-water deposits, ostracods look promising. Because of the great extent of Jurassic in western and southern Europe and because of the long-continued study of the system here, this is by far the most detailed part of the book.

Arkell gives his conclusions to his world study of the Jurassic in part VIII. During Jurassic times, the Pacific, the North Atlantic, and Arctic oceans were open, and the Indian Ocean had a barrier from Madagascar to Ceylon and India. The South Atlantic Ocean is not bordered by any known Jurassic. In other words, except for the South Atlantic, all the primordial oceans were open and much as they are today. Arkell rejects the 1911 marine realms or theories of Uhlig and devises for the Jurassic the following: (i) the Lias realm as a universal realm with world-wide distribution; (ii) the Pacific realm starting in the Toarcean, retreating in the Middle Bathonian; (iii) the Boreal realm spreading south from the Lower Callovian, retreating during the Upper Oxfordian, and again readvancing in the Lower Kimeridgian; and (iv) the Tethyan realm, which fluctuated in width throughout the system.

There is some speculation on the Jurassic climate, but except for the emphasis on the lack of known Jurassic icecaps, it adds little to the picture. The most active mobile belt during the Jurassic was around the Pacific, and the Tethys was the east-west mobile belt. Arkell rejects the complicated terminology of Kay for troughs and geosynclines, a step with which I agree. Arkell considers Stille's studies on diastrophism in the light of his (Arkell's) work on the Jurassic and in general finds that his own work supports the ideas of Stille. In connection with diastrophism, he states: "So far as our knowledge goes at present, it does not point to any master plan of universal, periodic, or synchronized orogenic and epeirogenic movements. The events were episodic, not periodic. There was no 'pulse of the earth.'" This last table, No. 28, gives four orogenies and six phases as named diastrophic episodes. A bibliography of 116 pages is arranged by regions.

This book ushers in a new approach to world geology as applied to marine faunas. (Similar work was done for Lower Cretaceous floras in 1911.) It represents an immense amount of detailed work. I know of no one who could have done a better piece of work on the Jurassic. This is an important contribution and sets up a very high standard of investigation for others to follow.

The publishers and the printers are also to be complimented—the work is excellent, the maps are readable and clean; the plates are fine-screen; and the type is clear.

E. WILLARD BERRY
Duke University

Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Observations on the Taxonomy, Biology and Ecology of the Engraulid and Clupeid Fishes in the Gulf of Nicoya, Costa Rica. Bull., vol. 1, No. 5. Clifford L. Peterson. 144 pp. *Studies of the Sexual Development and Spawning of Yellowfin Tuna (Neothunnus Macropterus) and Skip-Jack (Katsuwonus Pelamis) in Three Areas of the Eastern Pacific Ocean, by Examination of Gonads.* Bull., vol. 1, No. 6. Milner B. Schaeffer and Craig J. Orange. 69 pp. Inter-American Tropical Tuna Commission, La Jolla, Calif., 1956.

Radiation Field of an Oscillating Dipole–I. Notas de Física, vol. II, No. 11. Erasmo M. Ferreira. 20 pp. *Elastic Scattering of oc-Particles.* Notas de Física, vol. III, No. 1. S. W. MacDowell and J. J. Giambiagi. 4 pp. *Polarization of Spin One Particles by Nuclear Scattering.* Notas de Física, vol. III, No. 2. S. W. MacDowell and J. Tiomno. 14 pp. Centro Brasileiro de Pesquisas Físicas, Rio De Janeiro, 1956.

The Commonwealth Fund, Thirty-Eighth Annual Report for the Year Ending, June 30, 1956. Commonwealth Fund, New York, 1956. 61 pp.

Durability of Concrete. Highway Research Board Bull. 128. National Academy of Sciences–National Research Council, Washington, 1956. 50 pp. $0.90.

Notes on Some Intertidal Coleoptera with Descriptions of the Early Stages (Carabidae, Staphylinidae, Malachiidae). Trans. vol. XII, No. 11. Ian Moore. San Diego Society of Natural History, San Diego, Calif., 1956. 24 pp.

Oak Ridge Institute of Nuclear Studies, Tenth Annual Report. 30 June 1956. Oak Ridge Institute of Nuclear Studies, Oak Ridge, Tenn., 1956. 113 pp.

Unmeasured Hazards. An analysis of the effects of tests of atomic and thermonuclear weapons. World Federation of Scientific Workers, London, 1956. 40 pp. 2s.

John and Mary R. Markel Foundation, 1955–56 Annual Report. The Foundation, New York 17, 1956. 79 pp.

Meetings and Societies

Influence of Temperature on Biological Systems

The 11th annual meeting of the Society of General Physiologists was held 27–29 Aug. at the University of Connecticut, Storrs, in conjunction with the meetings of the AIBS. The first 2 days were devoted to a symposium organized by Frank H. Johnson on "The influence of temperature on biological systems," dedicated to A. V. Hill. The third day was given over to contributed papers and the business meeting.

The symposium was planned to deal with temperature comprehensively, at various levels of complexity ranging from the purely molecular to the organismic, and to bring together representative aspects of the general problem in current research. Because of the broad scope of the subject, it appeared desirable to have the symposium extend over at least two full days. Eighteen papers were presented and discussed in four consecutive sessions within this period.

The first session, David F. Waugh presiding, was primarily concerned with biological molecules: their formation, structure and stability, and reaction kinetics. In the opening paper, Henry Eyring, in collaboration with Frank Johnson, discussed some consequences of modern reaction rate theory with particular reference to the "organizational" type of reactions involved in biosynthesis, wherein the entropy change tends to be negative, as opposed to the "disorganizational" type of reactions, exemplified by denaturations, which proceed with positive entropy change. Reaction rate theory, together with the evidence pertaining to optical activity of biological molecules, leads to the conclusion that the change from a nonliving to a living world, a billion or so years ago, took place in a specific chemical event lasting 10^{-13} second (the lifetime of an activated complex), probably at a temperature not very different from the temperatures under which life exists today. In this critical event, an optically active template, possibly a ribonucleic acid molecule of sufficient length and configurational perfection, attained the property of reproducing itself, using the enzymes it could form and those present in the surrounding milieu, in a period shorter than its own lifetime.

Walter Kauzmann followed with a discussion of basic aspects of molecular structure as well as the significance of various experimental methods that can be employed to obtain information on structure and changes in structure of proteins. Two of the most convenient and rewarding methods are those of polarimetry and viscosity. Eugene Ackerman, in collaboration with G. K. Strother and R. L. Berger, reported that the rate at which beef liver catalase combines with H_2O_2 to form the intermediate compound is temperature independent from 5° to 45°C and is not appreciably altered by the dielectric constant of the medium but is dependent on rates of diffusion if they are below 0.8×10^{-5} square centimeters per second. The reaction rate of the intermediate compound with a second molecule of H_2O_2 is dependent on both temperature and diffusion rate. Britton Chance explained a simple equation by which the velocity constant for the combination of enzyme and substrate could be measured from the steady-state concentration of the enzyme-substrate complex and its half-life, no other data being necessary. The equation proved valid for the known kinetics of the peroxidase-H_2O_2 reaction and was used to study the effect of temperature on the extremely rapid reaction of oxygen with cytochrome a_3 of baker's yeast cells ($\tau 10^8 M^{-1} \times sec^{-1}$), and kinetic data were given for the range in temperature of 12° to 47°C.

In the final paper of this session, Leigh Chadwick reviewed the effects of temperature on the activity of cholinesterases. These enzymes are unusual in that, for the most part, their activity is directly proportional to temperature, up to the point where irreversible destruction becomes significant, and in that the thermal denaturation is not at all reversible, except in the cholinesterases of mammalian serum. Complexities in the rates of denaturation and the action of inhibitors are not yet adequately understood.

The second session, William D. McElroy presiding, dealt with the influence of hydrostatic pressure as well as temperature on the activity of specific enzymes and on the more complicated processes of muscle contraction, cell division, and ameboid motion. M. N. Swartz, in collaboration with N. O. Kaplan and Mary E. Frech, presented evidence that the activation of nucleotide-splitting enzymes (initially inactive in sonic extracts of various bacteria) by exposure to high temperatures involves the destruction of relatively heat-labile, apparently specific, inhibitors that have protein properties. The inhibitor of the *Mycobacterium* enzyme dissociates reversibly at acid *p*H, but that of the *Proteus* enzyme is destroyed at acid *p*H. The enzymes themselves undergo a reversible thermal denaturation. Karl F. Guthe discussed further evidence for the significance of reversible thermal denaturation in an extensive analysis of the influence of both temperature and hydrostatic pressure on the activity of myosin ATP-ase. Heat and alkalinity promote both the breakdown of the enzyme-substrate complex and the reversible denaturation of the catalyst, whereas increased pressure exerts the opposite influence. The quantitative effects of these factors are accounted for in terms of the heats and volume changes of activation and of reaction, respectively.

D. E. S. Brown presented the results of a prolonged study of pressure-temperature relationships of muscular contraction, culminating in elucidation of the role of these factors in controlling the contraction process. The evidence indicates that temperature and pressure affect tension through (i) the volume and thermal changes involved in actomyosin-complex activation and (ii) the rates of enzyme reactions involving phosphate donors. A basic role of myosin ATP-ase is suggested by an impressive similarity in values of the heats and volume changes associated with catalytic activity and reversible denaturation to the values resulting from analysis of the quantitative effects of temperature and pressure on glycerated psoas fibers and turtle auricle under various conditions.

D. A. Marsland surveyed the evidence bearing on the mechanism of cytokinesis in marine eggs, with special reference to the action of increased pressure or low temperatures in blocking cell division, and to quantitative changes in the pressure-temperature effects following the addition of ATP or mersalyl acid (salyrgan). The data indicate that cleavage results from contraction of a strongly gelled layer of cortical cytoplasm which undergoes solation at low temperatures or high pressures, and that, generally speaking, the gelation process is causally related to the development of protoplasmic contractility with the utilization of metabolic energy.

The third session, C. Stacy French presiding, included papers on cell division in bacteria and marine eggs, insect development, and plant processes. Victor G. Bruce reviewed recent work on synchronization of the division cycle in bacteria and protozoans, respectively. In

both types of organisms sudden changes in temperature away from the optimum appear to block a specific step that is necessary to initiate division, and if this step has been completed the temperature change does not block division. Ivor Cornman discussed the influence of temperature on the inhibitory action of various carbamates on cell division of *Arbacia* and *Echinarachnius* eggs. The effectiveness of a given drug concentration increased both above and below 20°–21°C with the former, and 16°C with the latter organism.

Glenn Richards presented data indicating that the amount of energy needed for development of the eggs of the milkweed bug *Oncopeltus fasciatus* increases as the temperature is lowered below 20°C. Nymphs hatched at just above the threshold temperature of 14°C usually die, even though they are placed under optimal growth conditions, and nymphs hatched at the optimal temperature of 25°C die if reared at 17°C; no evidence was found of pathology at the mitochondrial level, or of differences from normal oxygen consumption, or of deficiency in nutritional factors. Death could be prevented, however, by brief, daily exposures of eggs or nymphs to 25°C, suggesting that a temperature above 20°C is necessary for the synthesis of some vitality factor or for the elimination of an inhibitory influence.

F. W. Went discussed complexities in the influence of temperature on various processes in plants. The rate of protoplasmic streaming and of morphological differentiation is almost proportional to temperature. The temperature dependence of stem elongation and dry weight production is different, and the usually somewhat low optimum temperature varies with species, age, and size of the plant; in the tomato plant it varies also with light and darkness. Processes almost independent of temperature include the growth in length of young pea plants and the 24-hour rhythm of development of most plants. The effects of auxin and vitamins are influenced by temperature.

The fourth session, W. R. Amberson presiding, dealt primarily with the influence of temperature on some representative processes or phenomena in animal tissues, organs, and whole organisms but included also some highly interesting observations on pressure-temperature-inhibitor relationships in the activity of single nerve cells. F. Crescitelli noted differences in the effects of Na and K ions on the response of the A and B fiber groups in the bullfrog sciatic nerve at 1° to 5°C, as compared with 22° to 24°C. The interpretation was expressed that the optimum temperature for recovery of activity, on addition of Na ions after blockage by K ions, is lower in the B than in the A fibers, suggesting that differences in thermal responses of different fiber groups are characteristic of peripheral nerve fibers.

S. Spyropoulos, collaborating with I. Tasaki, reviewed the effects of temperature and pressure changes on amphibian single myelinated nerve fibers and the squid giant axon and reported new results obtained with internal stimulating and recording electrodes. The influence of temperature on certain properties of the membrane and axoplasm, as well as on the characteristics of the resting, excited, and recovery states, was discussed in detail. High hydrostatic pressure reversibly increased the duration of the action potential, by as much as fourfold. Fibers rendered inactive by certain narcotics at atmospheric pressure recovered their excitability in the presence of the narcotic at high hydrostatic pressure; narcotic potency was influenced also by temperature at normal pressure.

V. J. Wulff discussed rapid, transient changes in the absorption spectrum during bleaching of rhodopsin solutions at room temperature and slower changes that occur without bleaching at about 0°C. With grasshopper, *Limulus*, and frog eyes, Q_{10} values for the decrease, with rise in temperature, in latent period of the retinal action potential were found to range from 2.3 to 4, while the magnitude of the retinal action potential was much less sensitive to temperature. The data were interpreted in terms of a kinetic model in which an electrochemical process generating the action potential is coupled with a timing process controlling the duration of the latent period.

J. A. Miller presented extensive data concerning the influence of temperature on the resistance of animals to asphyxia; life was prolonged at lowered body temperatures and could be extended somewhat by the administration of adenosine phosphates. A. B. Otis described the effects of low body temperatures of anesthetized dogs on respiratory phenomena. The rate of ventilation is more sensitive to temperature than are oxygen consumption and CO_2 production, thus leading to hypoxia and acidosis at lowered temperatures. Without artificial ventilation, asphyxia occurs at 20°–25°C, but with artificial ventilation, circulation and transportation of respiratory gases, as well as their diffusion in the lungs and tissues, remain adequate at 15°C or lower, although ventricular fibrillation frequently sets in at temperatures below 25°C.

Abstracts of the symposium papers, as well as of the short research papers given on the third day, will appear as usual in the October issue of the *Journal of Cellular and Comparative Physiology*. The detailed papers of the symposium will be published as a monograph.

At the business meeting the membership approved the council decision to hold the 1957 annual meeting at the Marine Biological Laboratory, Woods Hole, Mass. The election of a representative to the Division of Biology and Agriculture of the National Research Council was approved.

ABRAHAM M. SHANES
National Institutes of Health, Bethesda, Maryland

FRANK H. JOHNSON
Princeton University, Princeton, New Jersey

Programs Planned for the AAAS New York Meeting

A number of outstanding programs have been planned for the forthcoming AAAS meeting in New York. Briefly described here are the programs in biochemistry, biology, botany, zoology, medicine, dentistry, and psychology, which are the programs of particular interest to experimental biologists. Also announced here are the programs in engineering, history and philosophy of science, and the social and economic sciences.

Programs previously announced are the special sessions, mathematics, physics, chemistry, and the earth sciences.

Biochemistry

Section C. Contributed papers in biochemistry; 26 Dec., afternoon and evening.

Symposium, cosponsored by the American Society of Naturalists and the Society of General Physiologists: "Chemical and biological aspects of cellular competition"; arranged by Werner Braun, Rutgers University, who will preside; 27 Dec., morning. Mechanisms of selective inhibition and stimulation, J. Gots, University of Pennsylvania; Some aspects of phagebacterium interactions, G. Streisinger, Carnegie Institution; Factors controlling competition in bacterial populations, Werner Braun; Selective inhibition as an element in cellular differentiation of animals, S. M. Rose, University of Illinois; Substances with differential effects on normal and neoplastic cells, J. J. Biesele, Sloan-Kettering Institute.

Symposium: "Biosynthesis of isoprenoid compounds"; arranged by H. Boyd Woodruff, Merck Sharp & Dohme Research Laboratories, who will preside; 28 Dec., morning. Steroid biosynthesis: chemical aspects, Samuel Gurin, University of Pennsylvania; Steroid biosynthesis: biological implications, Oscar Hechter, Worcester Foundation for Experimental Biology; Mevalonic acid in the biosynthesis of steroids, P. A. Tavormina, Margaret H. Gibbs, J. W. Huff, and L. D. Wright, Merck Sharp & Dohme Research Laboratories; Synthesis of rub-

ber by microorganisms, W. D. Stewart, M. R. Stewart, and Cedric Bielawski, Atlantic Research Corporation.

American Association of Clinical Chemists. Symposium: "Significant trends in clinical enzymology"; arranged by Harry Goldenberg, Hillside Hospital, Glen Oaks, N.Y., who will preside; 29 Dec., morning. Serum phosphohexose isomerase and other serum enzyme activities in cancer, Oscar Bodansky, Memorial Center for Cancer and Allied Diseases; Recent advances in enzyme methodology, Harry Goldenberg; Biochemical biopsy, Felix Wroblewski, Memorial Center for Cancer and Allied Diseases.

Biological Sciences

New York Academy of Sciences, cosponsored by Sections F and G. Two-session symposium: "Modern ideas on spontaneous generation"; arranged by a committee, Ross F. Nigrelli, New York Zoological Society, chairman; 26 Dec., morning and afternoon. Part I, Harold F. Blum, Princeton University, presiding; greetings from the academy, Hilary Koprowski, New York Academy of Sciences. Formation of organic compounds on the primitive earth, Stanley L. Miller, College of Physicians and Surgeons, Columbia University; Paleobiochemistry, Philip H. Abelson, Carnegie Institution of Washington; Electrolytic requirements of protists and archeo-metabolism, Seymour H. Hutner, Marvin Sanders, John A. McLaughlin, and S. Scher, Haskins Laboratories; Speculations on origins and evolution in photosynthesis by Sam Granick, Rockefeller Institute for Medical Research; Concluding remarks, Harold F. Blum. Part II, George Wald, Harvard University, presiding. Phosphorus and the origin of life, Addison Gulick, Cambridge, Mass.; Interaction of synthetic polynucleotides, Robert C. Warner, New York University College of Medicine; Structure of crystalline proteins, David Harker, Polytechnic Institute of Brooklyn; Spontaneous generation of protein and anabolic pathways, Sidney W. Fox, Florida State University; The gene as the prime mover, Carl C. Lindegren, Southern Illinois University; Concluding remarks, George Wald.

American Museum of Natural History, cosponsored by Sections F and G. Demonstrations of methods of museum preparation and exhibition; 26 Dec., morning. Introductory remarks by Gordon Reekie, American Museum of Natural History, and tour of exhibits in preparation.

Symposium: "Museum techniques"; arranged by Gordon Reekie; 26 Dec., afternoon. Speakers: Lester R. Aronson, Edwin H. Colbert, Dean Amadon, and Lothar Witteborg, American Museum of Natural History.

Ecological Society of America. Contributed papers in plant ecology, cosponsored by Section G; 27 Dec., morning. Contributed papers in animal ecology, cosponsored by Section F; 27 Dec., afternoon.

Symposium, cosponsored by Sections H and L and the New York Academy of Sciences, Section on Anthropology: "Values in human ecology"; arranged by George B. Happ, Principia College, who will preside; 28 Dec., morning. The expansion of the human ecosystem, Stanley A. Cain, University of Michigan; Cultural transformations in ecological perspective, Margaret Mead, American Museum of Natural History; An approach to integration of the biological and social sciences in human ecology, George B. Happ; Summary and comments, Paul B. Sears, Yale University.

Two-session symposium, cosponsored by the Association of American Geographers: "Social significance of ecological research"; arranged by Murray F. Buell, Rutgers University. Part I, cosponsored by Section G, "Plant ecology," John E. Cantlon, presiding; 29 Dec., morning. Natural and cultural aspects of the flood problem, Paul B. Sears, Yale University; Supermarket ecology, Jack McCormick, American Museum of Natural History; The breath of the land, Stuart B. LeCompte, Jr., Rutgers University; Ice storm ecology, Paul C. Lemon, State College for Teachers, Albany, N.Y.; The suburban forest, Jerry Olsen, Connecticut Agricultural Experiment Station; Principles of sound right-of-way vegetation management, William A. Niering, Connecticut College; Conflict between the shellfish and duck industries of Long Island, a praetical problem in marine ecology, John H. Ryther, Woods Hole Oceanographic Institution. Part II, cosponsored by Section F, "Animal ecology," Paul G. Pearson, Rutgers University, presiding; 29 Dec., afternoon. A contribution to the natural oyster-bed problem, Harold H. Haskin, Rutgers University; Development of areas of low-quality soils for wildlife and public use in southern New Jersey, L. G. MacNamara, New Jersey State Department of Conservation and Economic Development; An ecological survey instigated by the disappearance of an animal used commonly in biological research, John S. Rankin, Jr., University of Connecticut; The evening flights of water fowl, Jeff Swinebroad, Rutgers University; Endocrine responses in rats and mice to increasing population size with special emphasis on the delayed effects on offspring, J. J. Christian, Naval Medical Research Institute; Some effects of pollution on the ecological and biological aspects of the stream, Ruth Patrick, Academy of Natural Sciences of Philadelphia.

Sections F and G and the Genetics Society of America. Symposium: "Biochemistry of the cell nucleus"; arranged by Arthur W. Pollister, Columbia University, who will preside; 28 Dec., morning. Correlation of desoxyribose nucleic acid content with chromosome complement, Cecilie Leuchtenberger, Western Reserve University; Time of synthesis of desoxyribose nucleic acid, J. Herbert Taylor, Columbia University; Changes in the interphase nucleus with cellular function, Max Alfert, University of California; Chemical studies of the giant chromosomes of Diptera, George T. Rudkin, Institute for Cancer Research.

Zoologists' luncheon and vice-presidential address, "In pursuit of a gene," Bentley Glass, Johns Hopkins University; 28 Dec.

Sections F, G, and I. Two-session symposium: "Problems of aging"; arranged by H. J. Curtis, Brookhaven National Laboratory, Paul J. Kramer, Duke University, and Conrad G. Mueller, Columbia University. Part I, Paul J. Kramer, presiding; 29 Dec., morning. Aging in lower organisms, A. I. Lansing, University of Pittsburgh; Physiological aspects of aging in plants, William J. Robbins, New York Botanical Garden; Aging and regeneration in plants, Albert L. Delisle, Sacramento State College; Physiological aspects of aging in man, N. W. Shock, National Institutes of Health. Part II, H. J. Curtis, presiding; 29 Dec., afternoon. Aging in human populations, Hardin B. Jones, University of California; Radiation-induced aging, H. A. Blair, University of Rochester; Psychological aspects of aging, James E. Birren, National Institutes of Health; Aging in humans, Irving Lorge, Columbia University.

Society for the Study of Evolution. Contributed papers; 27 and 28 Dec., mornings and 29 Dec., afternoon.

Symposium, cosponsored by the Society of Vertebrate Paleontology and the American Society of Naturalists: "Biotic communities in the past and today"; arranged by Harlan Lewis, University of California, Los Angeles; 29 Dec., morning; Jens C. Clausen, Carnegie Institution of Washington, Stanford, presiding. Genetic variability in relation to environment, Jens C. Clausen; Migrations of Cenozoic forest communities in North America, Erling Dorf, Princeton University; Rise of the grass-eating mammals, Joseph T. Gregory, Yale University; Man changing the environment, Paul B. Sears, Yale University.

Mountain Lake Biological Station. Breakfast for all former students, investigators, and staff; 28 Dec., morning.

Botanical Sciences

Section G. Contributed papers on general botany, cosponsored by the Botanical Society of America, joint with the

Torrey Botanical Club, 27 Dec., morning.

Botanists' dinner and vice-presidential address, "Some implications of the concept of outer space," Paul J. Kramer, Duke University, in celebration of the 50th anniversary of the founding of the Botanical Society of America. Remarks by Harriet B. Creighton, Wellesley College, president of the society.

Symposium, cosponsored by Section F and the Botanical Society of America: "Some unsolved problems in biology"; Rollin D. Hotchkiss, Rockefeller Institute for Medical Research, presiding; 28 Dec., morning. Regulatory mechanisms in cellular metabolism, Bernard D. Davis, New York University College of Medicine; How do genetic agents act? Barry Commoner, Washington University; Forms of life more primitive than those now in existence, Alfred E. Mirsky, Rockefeller Institute for Medical Research; The chromosome as genetic and paragenetic mechanism, Kenneth W. Cooper, University of Rochester; Discussion.

Contributed papers on plant physiology, cosponsored by the American Society of Plant Physiologists, the Botanical Society of America, and the Society of General Physiologists; 30 Dec., morning.

Mycological Society of America, cosponsored by Section G. Symposium: "Genetics of the fungi"; arranged by Lindsay S. Olive, Columbia University, and Alma W. Barksdale, New York Botanical Garden; 26 Dec., afternoon; Haig Papazian, New Haven, Conn., presiding. Evolution of heterothallism in the ascomycetes, Harry Wheeler, Louisiana State University; Nuclear phenomena associated with heterocaryosis, caryogramy, and segregation in the homothallic ascomycete *Sclerotinia trifoliorum* Eriksson, Alec J. H. Carr, University College of Wales; Gene conversion in fungi, Patricia St. Lawrence, Yale University; Genetic control of mating-type in fungi, Haig Papazian.

Zoological Sciences

International Union for the Study of Insects, North American Section. Two-session symposium: "Communication in insects"; arranged by T. C. Schneirla, American Museum of Natural History. Part I: "Perspective on fact and theory"; William S. Creighton, City College of New York, presiding; 26 Dec., afternoon. Communicative dancing by insects, Vincent G. Dethier, Johns Hopkins University; Insect communication by the medium of food distribution, Edward O. Wilson, Harvard University; Mechanisms of communication in ants, Arthur C. Cole, University of Tennessee; Contrasting patterns in ants, and theoretical remarks, T. C. Schneirla, American Museum of Natural History. Discussants: J. A. Downes, Science Service, Ottawa, Canada, Neal A. Weber, Swarthmore College, and John B. Calhoun, National Institute of Mental Health. Part II: "Problems and methods"; Kenneth D. Roeder, Tufts University, presiding; 27 Dec., morning. Chemoreceptive mechanisms, Edward S. Hodgson, Columbia University; Sensory factors in the orientation of moths, Ilse Schwinck, New York University College of Medicine; Phonoreception, Asher E. Treat, City College of New York; Steering mechanisms, Horst Mittelstaedt, Max-Planck Institut, Germany. Discussant: William Van der Kloot, Harvard University.

Entomological Society of America. Address, "What have we learned from the codling moth?" B. A. Porter, U.S. Department of Agriculture; 27 Dec., morning. Concurrent sessions for contributed papers, 27–30 Dec., mornings and afternoons, and evening, 28 Dec.

Symposium: "Teaching entomology"; H. L. Sweetman, University of Massachusetts, presiding; 27 Dec., afternoon. Teaching of biology to precollege students, I. E. Wallen, AAAS Science Teaching Improvement Program; Influence of museums and entomological clubs, Lucy W. Clausen, American Museum of Natural History; Techniques in teaching, L. A. Hetrick, University of Florida; Comparison of teaching methods of Canada and the United States, A. S. West, Jr., Queens University, Toronto; Canadian entomological training, F. O. Morrison, MacDonald College; In-service training programs in the plant quarantine branch, Agricultural Research Service, Ira A. Lane, Plant Quarantine Branch, USDA; Entomology in the armed services, K. L. Knight, U.S. Navy; Summary of fitness of entomological trainees, S. B. Freeborn, University of California.

Symposium: "The nematode situation"; F. A. Soraci, New Jersey Department of Agriculture, presiding; 27 Dec., afternoon. An evaluation of the nematode problem, Albert L. Taylor, Horticultural Crops Research Branch, USDA; Status of the golden nematode, Emory D. Burgess, Plant Pest Control Branch, USDA; Soybean nematode in North Carolina, J. N. Sasser, North Carolina State College; The burrowing nematode situation in Florida, Wray Birchfield, State Plant Board of Florida; Summarization, W. Lee Popham, Crops Regulatory Programs.

Entomologists' mixer; 27 Dec., evening.

Address, "Status of yellow fever in the Americas," Fred L. Soper, Pan-American Sanitary Bureau; 28 Dec., morning.

Invitation program: "Insect attractants"; Louis M. Roth, Quartermaster Research and Development Center, presiding; 28 Dec., afternoon. The physiological basis for insect attraction, V. G. Dethier, Johns Hopkins University; Host selection by phytophagous insects, A. J. Thorsteinson, University of Manitoba; Host finding by blood-sucking arthropods, E. R. Willis, Quartermaster Research and Development Center; Studies on the sex attractant of the American cockroach, D. R. A. Wharton, Quartermaster Research and Development Center.

Symposium: "Museums and their problems"; D. J. Borrer, Ohio State University, presiding; 28 Dec., evening. General statements of museum objectives, A. B. Grobman, Florida State Museum; Responsibilities of the curator of insects, T. H. Hubbell, University of Michigan; Responsibilities of the department of education, J. R. Saunders, American Museum of Natural History; Museum publication policies, John S. Lea, U.S. National Museum; Museum financial and personnel problems, A. E. Parr, American Museum of Natural History; Future of museums, A. S. Romer, Museum of Comparative Zoology, Harvard University.

Address, "Insect hosts of plant viruses," Karl Maramorosch, Rockefeller Institute for Medical Research; discussant, M. F. Day, Australian Embassy; 28 Dec., evening

Invited papers; 29 Dec., morning. Role of insect pathology in biological control, C. G. Thompson, Entomology Research Branch, USDA; Biological control of forest insects, P. B. Dowden, Northeastern Forest Experiment Station.

Address, "Some aspects of insect flight," B. Hocking, University of Alberta; 29 Dec., afternoon.

Invited paper, "Perspectives in insect endocrinology and tissue culture," Howard A. Schneiderman, Cornell University; discussant, William Trager, Rockefeller Institute for Medical Research; 29 Dec., evening

Symposium, cosponsored by the Society of Systematic Zoology: "The role of insects in nature"; D. J. Borrer, Ohio State University, presiding; 30 Dec., morning. Physiology and its contributions, K. D. Roeder, Tufts University; Systematics and evolution, H. H. Ross, University of Illinois; Insect contributions to genetics, Th. Dobzhansky, Columbia University; The numbers of insects, Curtis Sabrosky, Entomology Research Branch, USDA; Value of insects, D. M. DeLong, Ohio State University.

Symposium "The fate of insecticides in plants and animals"; Y. P Sun, Shell Oil Company, presiding; 30 Dec., morning. Comparative metabolic fates of DDT, H. H. Moorefield, Boyce Thompson Institute of Plant Research; Rate of storage and excretion of DDT in the rat, W. J. Hayes and W. F. Durham, U.S. Public Health Service; Metabolism of

organophosphorus insecticides by certain microorganisms, Mostafa Kamal Ahmed and John E. Casida, University of Wisconsin; Intermediary metabolic studies with DDT in rats, evaluation of residue toxins of Systox and Thimet in alfalfa, and the effects of feeding Systox residue toxins to cows and poultry, Paul A. Dahm, Iowa State College.

Society of Systematic Zoology. Contributed papers; 29 Dec., afternoon.

Society of Vertebrate Paleontology. Technical sessions; 28–30 Dec., mornings and afternoons.

Medical Sciences

Section N. Four-session symposium: "Evolution of nervous control from primitive organisms to man"; arranged by Bernard B. Brodie, National Heart Institute. Part I, 29 Dec., morning; Chester Yntema, State University of New York Upstate Medical Center, presiding. A common basis for development and behavior in organisms, Edmund W. Sinnott, Yale University; Chemical inducers in embryonic development, M. C. Niu, Rockefeller Institute for Medical Research; Functional and anatomic development of nervous system in lower animals, C. Ladd Prosser, University of Illinois. Part II, 29 Dec., afternoon; Abraham Shanes, National Institutes of Health, presiding. Conduction of nerve impulses, Harry Grundfest, College of Physicians and Surgeons, Columbia University; Neurohumoral agents as mechanism of nervous integration, G. B. Koelle, University of Pennsylvania; Organization of sensory and motor systems in mammalian cerebral cortex, Clinton N. Woolsey, University of Wisconsin. Part III, 30 Dec., morning; Bernard B. Brodie, presiding. Physiology of emotion, Keith Killam, University of California Medical Center, Los Angeles; Comparative approach to study of drug effects on behavior of higher animals, Joseph Brady, Walter Reed Army Medical School of Research; vice-presidential address of Section N, "Problems involved in the biochemical approach to brain function," Irvine H. Page, Cleveland Clinic Foundation. Part IV, 30 Dec., afternoon; Nathan S. Kline, Rockland State Hospital, Orangeburg, N.Y., presiding. Alteration of behavior in man after brain injuries, H. L. Teuber, New York University; Biological aspects of mental disease, Samuel Bessman, University of Maryland Medical School; Application of the experimental method to psychoanalytical theory, I. Arthur Mirsky, University of Pittsburgh School of Medicine. A discussion period will follow each group of papers and, at one of the sessions, announcements will be made of the winners of the 12th Theobald Smith award in the medical sciences, the second AAAS-Anne Frankel Rosenthal memorial award for cancer research, and the first AAAS-Ida B. Gould memorial award for research on cardiovascular problems.

Alpha Epsilon Delta, cosponsored by Sections C, F, and N, and Beta Beta Beta Biological Society. Symposium: "Problems in premedical education"; arranged by Maurice L. Moore, Bronxville, N.Y.; 29 Dec., morning. Introductory remarks, Lloyd R. Gribble, West Virginia University; Study of arts and science in the premedical curriculum, William N. Hubbard, Jr., New York University College of Medicine; A medical school admissions committee's comments on premedical education, Lawrence W. Hanlon, Cornell University Medical College; Premedical education and the liberal arts college—a report on the "Oberlin study," Warren F. Walker, Oberlin College; Appraisal of applicants to medical school—a report on the 1956 Institute of the Association of American Medical Colleges, John T. Cowles, University of Pittsburgh School of Medicine; Criteria for admission to medical school—a panel discussion on the 1956 AAMC Institute, Hugh E. Luckey, Cornell University Medical College; John T. Cowles; George E. Miller, University of Buffalo; William E. Cadbury, Jr., Haverford College; Norman F. Witt, University of Colorado.

Luncheon and address, "Methods of improving liaison and cooperation between medical and liberal arts colleges," Joseph C. Hinsey, New York Hospital–Cornell Medical Center; 29 Dec., noon. Tour through the Cornell University Medical College and Hospital; 29 Dec., afternoon.

American Association of Hospital Consultants. Symposium: "The hospital and the sequence of care"; arranged by Jacque B. Norman, Greenville, S.C.; 27 Dec., morning; Jack Masur, U.S. Public Health Service, presiding. Hospital facilities in relation to degree of illness: intensive nursing care, normal nursing care, and self-help care, Vane M. Hoge, U.S. Public Health Service. Discussants: John E. Gorrell, National Foundation for Infantile Paralysis; Morris Hinenburg, Federation of Jewish Philanthropics of New York. The relation of convalescent home to hospital care, George Bugbee, Health Information Foundation, Inc. Discussants: Ray E. Trussell, Columbia University; E. Dwight Barnett, Columbia University. The role of home care, Basil C. MacLean, Commissioner of New York City Hospitals. Discussants: E. M. Bluestone, consultant, formerly director, Montefiore Hospital, New York; Peter Rogatz, Health Insurance Plan of Greater New York.

American Physiological Society. Panel: "Recruitment and training of biological scientists"; 28 Dec., afternoon; Fred A. Hitchcock, Ohio State University, presiding. Panel members: William R. Amberson, University of Maryland Medical School; John P. Harrold, Midland (Mich.) Senior High School; Louis Levin, National Science Foundation; Ross A. McFarland, Harvard School of Public Health; Walter C. McNelly, Miami University; C. Ladd Prosser, University of Illinois.

American Psychiatric Association, cosponsored by the American Public Health Association. Four-session symposium commemorating the centennial of the birth of Emil Kraepelin: "Epidemiology of mental disorder"; arranged by Benjamin Pasamanick, Ohio State University Health Center. Part I, 27 Dec., morning; Paul Hoch, New York State Department of Mental Hygiene, presiding. The Emil Kraepelin memorial lecture, Eugen Kahn, Baylor University College of Medicine; Factors related to personality change during the second decade in the lives of young people, A. R. Mangus and E. Z. Dager, Ohio State University; The relations of schizophrenia to the social structure of a small city, J. A. Clausen and M. L. Cohn, National Institute of Mental Health. Part II, 27 Dec., afternoon; Paul Hoch, presiding. Complication of pregnancy among prenatal patients reporting previous nervous illness, D. G. Wiehl, K. Barry, and W. T. Tompkins, Millbank Fund; Epidemiological aspects of prognosis in mental illness, J. Zubin, E. I. Burdock, and S. Sutton, New York State Psychiatric Institute; The housing environment and mental health, D. M. Wilner and R. P. Walkley, Johns Hopkins University. Part III, 28 Dec., morning; Paul V. Lemkau, New York City Community Health Board, presiding. Treated and untreated mental disorders in the metropolis, L. Srole and T. S. Langner, New York Hospital; A survey of mental disorder in an urban population, II: prevalence by socio-economic status and race, B. Pasamanick, Ohio State University; D. Roberts, National Association for Crippled Children and Adults; P. V. Lemkau, New York City Community Health Board; and D. Krueger, Commission on Chronic Illness. A survey technique for estimating the prevalence of psychoneurotic and related types of disorders in communities, A. MacMillan and A. Leighton, Cornell University. Part IV, 28 Dec., afternoon; Paul V. Lemkau; presiding. Genetic and demographic aspects of disordered behavior patterns in a deaf population, J. D. Rainier and E. J. Kallmann, New York State Psychiatric Institute; Distribution of intellectual potential in an infant population, H. Knobloch and B. Pasamanick, Ohio State University, and P. A Harper and R. Rider, Johns Hopkins University; An investigation of seasonal variation of mental hospitalization

for old-age psychoses, I. McCaffrey, J. Downing, and E. Rogot, Community Mental Research Unit, Syracuse, N.Y.

Dentistry

Section Nd. Contributed and invited papers; 28 Dec., morning.

Two-session symposium, cosponsored by Sections H and N and the American Academy of Forensic Sciences: "The human dentition in forensic medicine"; arranged by W. M. Krogman, University of Pennsylvania, who will preside. Part I, 29 Dec., morning. Determination of personal identification by means of the teeth, Robert D. Wyckoff, Bureau of Medicine and Surgery, Department of the Navy; Criteria for age determination by means of teeth and identification of fragmentary teeth, David Scott, National Institutes of Health; Calcification pattern of human teeth, Maury Massler, University of Illinois; Time and sequence of tooth eruption, V. O. Hurme, Forsythe Dental Infirmary for Children, Boston, Mass. Part II, 29 Dec., afternoon. Criteria of individuality in the teeth, A. A. Dahlberg, University of Chicago; A survey of racial traits in the human dentition, Gabriel Lasker, Wayne University School of Medicine; Genetics of the human dentition, B. S. Kraus, University of Arizona; Roentgenographic appraisal of cephalo-facio-dental individuality, Viken Sassouni, Institute for Child Study of Philadelphia.

Symposium, cosponsored by Sections C, N, and Np: "Antienzymes"; arranged by a committee, George C. Paffenbarger, American Dental Association Research Division, National Bureau of Standards, chairman; 29 Dec., afternoon; Ed. F. Degering, Quartermaster Research and Development Center, presiding. Insulinase inhibitors, I. Arthur Mirsky, University of Pittsburgh School of Medicine; Antimetabolites and semienzymes, D. W. Woolley, Rockefeller Institute for Medical Research; Chemical aspects of enzyme inhibition, Irwin W. Sizer, Massachusetts Institute of Technology; Organic structures capable of inhibiting bacterial glycolysis, R. S. Manly, Tufts University.

Psychology

Section I. Invited papers: "Sensory processes"; arranged by Floyd Ratliff, Rockefeller Institute for Medical Research; 29 Dec., morning. Lorrin A. Riggs, Brown University, presiding. Physiological properties of olfactory receptors, Lloyd Beidler, Florida State University; Inhibitory interaction in the eye, Floyd Ratliff and H. K. Hartline, Rockefeller Institute for Medical Research; Mechanics of the internal ear, Georg v. Békésy, Harvard University.

Invited papers: "Advances in experimental psychopathology"; arranged by Robert Patton, University of Pittsburgh, who will preside; 29 Dec., afternoon. Precept and proficiency: a neuropsychological analysis, Karl Pribram, Institute of Living, Hartford, Conn.; Biometrics of psychopathology, Eugene Burdock, Samuel Suffon, and Joseph Zubin, Columbia University; Psychopathological significance of cerebral stimulation of animals and human beings, Jose M. R. Delgado and H. Hamlin, Yale University, and Y. D. Koskoff, Montefiore Institute of Research, Pittsburgh.

Vice-presidential address, "Color defect and fundamental visual processes," Clarence H. Graham, Columbia University; 29 Dec., afternoon.

Engineering

Section M. Three-session symposium, cosponsored by Sections C, H, I, K, L, and N and the Conference on Scientific Manpower: "Aids for environmental control"; arranged by Eugene F. Murphy, Prosthetic and Sensory Aids Service, Veterans Administration, and Irving P. Orens, Newark College of Engineering. Part I, "Overcoming normal and abnormal physical limitations"; 26 Dec., morning; Eugene F. Murphy, presiding. Ability and limitations of normal man, Max W. Lund, Office of Naval Research; Instrumentation for bioengineering, Wallace E. Frank, Franklin Institute; Some biomechanical methods for evaluating activities, Anthony Staros and William Romahn, Veterans Administration, and Rudolfs Drillis, New York University. Discussant: Renato Contini, New York University. Part II, "Extending mental and rational powers"; 26 Dec., afternoon; Irving P. Orens, presiding. Biomechanics in crash mortality: the surgical approach coordinating man's built-in crash defenses, Chas. Murray Gratz, New York, N.Y.; Extending mental processes by analogs, John R. Ragazzini, Columbia University; Harnessing the digital computers, R. W. Hamming, Bell Telephone Laboratories. Discussant: Frederick A. Russell, Newark College of Engineering. Part III, "Breaking the language barriers"; 27 Dec., morning; John Lotz, Columbia University, presiding. Translating teletypesetter tape to braille, Sidney Friedrich, Republic Aviation Corporation; Reduction of speech signals for easier transmission, Sze-Hou Chang, Northeastern University; Mechanical translation of German, Victor H. Yngve, Massachusetts Institute of Technology; Mechanical translation of Russian, Anthony G. Oettinger, Harvard University. Discussants: Charles Ritter, American Foundation for the Blind; F. S. Cooper, Haskins Laboratories; Kenneth N. Stevens, Massachusetts Institute of Technology; Homer Dudley, Bell Telephone Laboratories; G. S. Fielstra, New York Public Library.

History and Philosophy of Science

Section L. Contributed papers; 26 Dec., morning.

Two-session symposium, cosponsored by the Philosophy of Science Association: "Science and ethics"; arranged by Joseph Mayer, Miami University. Part I, 26 Dec., afternoon; Margaret Mead, American Museum of Natural History, presiding. Development of ethical and related concepts, Joseph Mayer, Miami University; Scientific Approach to ethics and its consequences, Anatol Rapoport, University of Michigan; Naturalistic view of ethics, Patrick Romanell, University of Texas; Ethics and the science of law, Samuel E. Stumpf, Vanderbilt University. Part II, 26 Dec., evening; Ralph W. Gerard, University of Michigan, presiding. Place of facts in a world of values, John Dashiell, University of North Carolina; A naturally operative ethical principle, Chauncey Leake, Ohio State University; Valuing in cross-cultural perspective, Margaret Mead; Biological basis of ethical values, Edmund W. Sinnott, Yale University.

Symposium, joint with the American Philosophical Association, Eastern Division: "The general significance of the work of Freud"; arranged by John Wild, Harvard University, and Jane M. Oppenheimer, Bryn Mawr College; 27 Dec., morning; Lawrence S. Kubie, Yale University School of Medicine, presiding. Freud in the history of science, Ernst Kris, New York, N.Y.; The indictment of Western philosophy in Freudian theory, Herbert Marcuse, Brandeis University; Freud's science and the exercise of self-consciousness, Philip Rieff, Brandeis University. Discussants: John A. Irving, University of Toronto; James G. Miller, University of Michigan.

Dinner, joint with the History of Science Society; presidential address of the History of Science Society, "Four years in retrospect," Dorothy Stimson, Stonington, Conn.; vice-presidential address of Section L, "Critics of science: friendly and unfriendly," Henry Guerlac, Cornell University; 28 Dec., evening.

History of Science Society, cosponsored by Section L. Symposium: "The interaction of science and technology"; arranged by Pearl Kibre, Hunter College; 28 Dec., morning; Henry Guerlac, Cornell University, presiding. Interaction of science and technology in the development of metallurgy, Cyril S. Smith, University of Chicago; Coal-tar dye manufacture and the origins of the modern industrial research laboratory, John J. Beer, Hanover College; History of the wave concept from Vitruvius to Newton

and Huygens, Frederick Kilgour, Yale University Medical School; Comment, Bern Dibner, Burndy Engineering Library, Norwalk, Conn.

Luncheon and address, "Report on the International Congress of the History of Science," I. Bernard Cohen, Harvard University; 28 Dec., noon.

Session on "Studies in modern science"; 28 Dec., afternoon; R. B. Lindsay, Brown University, presiding. Some considerations on the history of 17th-century chemistry, Marie Boas, Brandeis University; Caloric theory of adiabatic compression, Thomas S. Kuhn, University of California; Role of the U.S. Navy in creating a national agency for aeronautical research, Lee M. Pearson, Department of the Navy. Comment, Henry Noss, New York University.

Session on "Studies on medieval science"; 29 Dec., morning; Chauncey D. Leake, Ohio State University, presiding. Impact of Archimedes on medieval science, Marshall Clagett, University of Wisconsin; Medieval theory of supposition: whence and why? Curtis Wilson, St. John's College; Whose authority dominated medieval science? William H. Stahl, Brooklyn College; Comment, Edward Rosen, City College of New York.

Session on "History of Medicine"; 29 Dec., afternoon; John F. Fulton, Yale University School of Medicine, presiding. The transition from Egyptian to Greek medical theory by J. B. deC. M. Saunders, University of California School of Medicine; *Airs, Waters, and Places* in history, Genevieve Miller, Western Reserve University; *De Complexionibus,* Lynn Thorndike, Columbia University.

Philosophy of Science Association, cosponsored by Section L and the American Philosophical Association. Five-session symposium: "Measurement." Part I, "Measurement in the physical sciences"; arranged by Henry Margenau, Yale University, who will preside; 29 Dec., morning. Are physical magnitudes operationally definable? A. Pap, Yale University; Quantum theoretical concept of measurement, J. L. McKnight, Yale University; Definition and measurement in physics, P. Caws, Michigan State University. Part II, "Measurement in the social sciences"; arranged by Paul Lazarsfeld, Columbia University, who will preside; 29 Dec., afternoon. Disposition concepts, Ernest Nagel, Columbia University; Role of probability models in social science measurement, Frederic Mosteller, Harvard University, and Paul Lazarsfeld; Derivation of measurement from a system of axioms, Duncan Luce, Columbia University. Part III, "Measurement in the value sciences"; arranged by Donald Davidson, Stanford University, who will preside; 29 Dec., evening. Part IV, "Formal aspects of measurement"; arranged by Philburn Ratoosh, Ohio State University, who will preside; 30 Dec., morning. Mensuration and functional connection, Karl Menger, Illinois Institute of Technology; Theory of scales, with application in psychophysics, S. S. Stevens, Harvard University. Part V, "General aspects of measurement," arranged by Sebastian B. Littauer, Columbia University, who will preside; 30 Dec., afternoon. Measurement of rare events, E. J. Gumbel, Columbia University; Components of measurement, C. West Churchman, Case Institute of Technology; Measurements and managerial decisions, Paul Kircher, University of California.

Society for the Advancement of General Systems Theory. Contributed papers; 29 Dec., afternoon.

Symposium: "Systems under stress": arranged by Anatol Rapoport, Mental Health Research Institute, University of Michigan; 30 Dec., afternoon; Stuart C. Dodd, University of Washington, presiding. The individual under stress, James G. Miller, University of Michigan; The small problem-solving group under stress, Anatol Rapoport.

Social and Economic Sciences

Section K, joint with the *National Academy of Economics and Political Science* and the *American Political Science Association,* with the collaboration of Pi Gamma Mu. Symposium: "Impact of natural science on social science"; arranged by Donald P. Ray, George Washington University; 26 Dec., evening; Harold D. Lasswell, Yale University, presiding. Scientific progress and political science, Bernard Brodie, RAND Corporation; Sociology and the advances of natural science, Pitirim A. Sorokin, Harvard University; Vice-presidential address of Section K: Effects of modern wars on the political economy, Benjamin H. Williams, Industrial College of the Armed Forces.

American Statistical Association, cosponsored by Section K. Symposium: "Labor mobility and earnings"; arranged by A. J. Jaffe, Columbia University; 27 Dec., afternoon; Meredith B. Givens, New York State Incomes Study, presiding. Relation of earnings and mobility in the case of Ph.D. scientists, Theresa R. Shapiro, Columbia University; Job opportunities and geographic migration, Maurice C. Benewitz, City College of New York. Discussant: Charles A. Pierce, New York State Department of Labor.

Symposium: "Statistics in public health"; arranged by Carl L. Erhardt, Department of Health, City of New York; 27 Dec., afternoon; George James, Department of Health, City of New York, presiding. Long-term evaluation of poliomyelitis vaccine, Morris Greenberg, Department of Health, City of New York; Morbidity, mortality, and industrial retirement, J. S. Tyhurst, McGill University; Accident morbidity statistics in New York City, Harold Jacobziner, Department of Health, City of New York; Incidence and prevalence rates for cancer of the cervix, Floyd County, Georgia, Herbert Nieburgs, Beth-El Hospital, Brooklyn, N. Y.; Relationship of syphilis to cancer of the cervix, Abraham Oppenheim and Jules Vandow, Department of Health, City of New York.

Pi Gamma Mu, National Social Science Honor Society. Dinner, in honor of the officers and speakers of Section K and the National Academy of Economics and Political Science; 26 Dec., evening.

Society for the Advancement of Criminology, cosponsored by Section K. Two-session symposium, joint with the Association for the Psychiatric Treatment of Offenders and the Institute for Research on Crime and Delinquency: "Science versus crime"; arranged by Donal E. J. MacNamara, New York Institute of Criminology, who will preside. Part I, "The social scientist's approach to crime phenomena"; 29 Dec., morning. Discussants: Melitta Schmideberg, Association for the Psychiatric Treatment of Offenders; Herbert Bloch, Brooklyn College. A scientific approach to the narcotics problem, Hubert Howe, New York Academy of Medicine. Clinical and practical problems in the rehabilitation of convicted offenders, Marcel Frym, Hacker Psychiatric Foundation; Race and crime: an analysis, Forrester Washington, New York, N.Y.; The sexual element in non-sex crimes, Albert Ellis, New York, N.Y.; Climatological and geographic factors affecting criminal behavior, Nicolaas Pansegrouw, Institute for Research on Crime and Delinquency; Crime trends under democratic and totalitarian governments, Donal E. J. MacNamara, New York Institute of Criminology. Part II, "Scientific advances in criminal investigation"; 29 Dec., afternoon. Discussants: Albert J. Genua, State Teachers College, Westfield, Mass.; Chauncey E. Smith, Mamaroneck, New York, Police Department. Exposing a documentary hoax, Martin K. Tytell, New York University, and Pearl Tytell, New York, N.Y.; Rebuttal, Isaac Don Levine, Waldorf, Md.; Chromatographic techniques in a crime laboratory, Mark Luckens, Hartford, Conn.; Scientific advances in interrogation: instrumental, hypnotic, and narcotic techniques, Cleve Backster, Backster Associates, Inc.; The Louisville experiment: effects of alcohol on driving capacity; Joseph J. Garbarino, New York Institute of Criminology; Photo-

graphic techniques in investigation, Vincent L. Stibler, New York Institute of Criminology.

Luncheon and panel, "Science versus crime"; 29 Dec., noon.

Anthropology

Section H. Symposium, cosponsored by the Society for American Archaeology: "American archeology"; arranged by Dorothy Cross Jensen, Hunter College; 26 Dec., afternoon; Robert H. Dyson, University of Pennsylvania, presiding. Cherokee acculturation and eastern woodlands, community typology, John Witthoft, Pennsylvania State Museum; Round houses in the western Arctic, J. L. Giddings, Jr., Brown University; Reliability of radiocarbon dates for late glacial and recent times, James B. Griffin, University of Michigan; The dimensions of archeology, A. C. Spaulding, University of Michigan; The Paleo-Indian in the Northeast, W. A. Ritchie, New York State Museum; A shorthand system for writing Meso-American dates and for comparing their correlation with the Christian calendar, Linton Satterthwaite, University of Pennsylvania; Report on the earliest cultural affiliation with amaranth seeds in North America, George A. Agogino and Sherwin Feinhandler, Syracuse University.

Symposium, joint with the American Institute of Human Paleontology: "Commemorating the 100th anniversary of the discovery of Neanderthal man"; arranged by Loren C. Eiseley, University of Pennsylvania, and William L. Straus, Jr., Johns Hopkins University; 27 Dec., morning; William W. Howells, Harvard University presiding. Neanderthal man and the dawn of human paleontology, Loren C. Eiseley; Archeological materials associated with Neanderthal man in Europe, Hallam L. Movius, Jr., Harvard University; Levalloiso-mousterian man in southwestern Asia and the Neanderthal problem, F. Clark Howell, University of Chicago; Some observations on the pathology of Neanderthal man, William L. Straus, Jr.; American Neanderthaloids, T. Dale Stewart, U.S. National Museum.

Vice-presidential address, I, "Prehistoric Asia and America—the northern route," James B. Griffin, University of Michigan; 27 Dec., afternoon; William L. Straus, Jr., Johns Hopkins University, presiding.

Contributed papers; 28 Dec., morning.

Luncheon and vice-presidential address, II, "The scientist and the pursuit of the good," W. Montague Cobb, Howard University; 28 Dec., noon; William L. Straus, Jr., presiding.

Two-session symposium: "Man in the tropics: the Caribbean"; arranged by Vera Rubin, Columbia University. Part I, 29 Dec., morning; George E. Simpson, Oberlin College, presiding. Introduction, Charles Wagley, Columbia University; Man-land relations in the Caribbean, Preston James, Syracuse University. Discussant: Jean Gottman, Institute for Advanced Study. Race relations in Caribbean culture history, Eric Williams, Historical Society of Trinidad and Tobago. Discussant: Frank Tannenbaum, Columbia University. African heritage in the Caribbean, Michael G. Smith, University College of the West Indies. Discussant: C. E. Simpson, Oberlin College. Part II, 29 Dec., afternoon; Franklin Frazier, Howard University, and Fernando Ortiz, University of Havana, presiding. The family in the Caribbean, Raymond T. Smith, University College of the West Indies. Discussant: John Murra, Vassar College. Plantation systems in the Caribbean by Elena Padilla, Columbia University. Discussant: Edgar T. Thompson, Duke University. Methods of community analysis in the Caribbean, Robert Manners, Brandeis University. Discussant: Conrad Arensberg, Columbia University. Present status of social science research in the British West Indies, Lloyd Braithwaite, University College of the West Indies; Cultural perspectives in Caribbean research, Vera Rubin, Columbia University.

Session for anthropological theory; 30 Dec., morning; Morton Fried, Columbia University, presiding. The determinants of local exogamy as exemplified by peoples of central Brazil, Gertrude E. Dole, New York, N.Y.; Simple coordinate system for the classification of the world's 3000 systems of values, Edward F. Haskell, Council for Unified Research and Education; Cultural innovation in a conservative society: aboriginals of Arnhem Land, Australia, Richard A. Waterman, Wayne University, and Patricia P. Waterman, Detroit, Michigan; A second look at Boas and Morgan, Eleanor Leacock, City College of New York.

Symposium: "Transitional communities in India, Pakistan, and Burma"; arranged by Gitel Poznanski Steed, New York, N.Y.; 30 Dec., morning; Dorothy Spencer, University of Pennsylvania, presiding. Indian tribal communities in transition, Elizabeth Bacon, Cornell University; Religious attitudes and values in a changing UP society, Jack Planalp, Human Relations Area Files, New Haven, Conn.; Secular influences on Hindu practices in rural Gujarat, Gitel Poznanski Steed, Research in Contemporary India Project, New York, N.Y.; The growth of urbanism in East Pakistan, Stanley Maron, Human Relations Area Files; Framework for a study of transitional communities in Burma, F. Kris Lehman, Columbia University.

Symposium: "Current studies in cultural evolution: Oceania"; 30 Dec., afternoon; Margaret Mead, American Museum of Natural History, presiding. Oceania and the problem of controls in the study of cultural evolution, Ward H. Goodenough, University of Pennsylvania; Differentiation by adaptation in Polynesian societies, Marshall D. Sahlins, Columbia University; Variations in Polynesian social organization, Irving Goldman, Sarah Lawrence College; Culture change and somatic variability in Melanesia, Douglas Oliver, Harvard University; A biologist comments, G. Evelyn Hutchinson, Yale University.

Forthcoming Events

December

26–31. American Assoc. for the Advancement of Science, annual, New York, N.Y. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5.)

27–28. Fluid Mechanics in Chemical Engineering, American Chemical Soc., Lafayette, Ind. (W. E. Ranz, Dept. of Engineering Research, Pennsylvania State Univ., University Park.)

27–28. Linguistic Soc. of America, Philadelphia, Pa. (A. A. Hill, Box 7790, University Sta., Austin 12, Tex.)

27–29. American Mathematical Soc., 63rd annual, Rochester, N.Y. (J. H. Curtiss, AMS, 80 Waterman St., Providence 6, R.I.)

27–29. American Physical Soc., Monterey, Calif. (W. A. Nierenberg, Univ. of California, Berkeley 4.)

27–29. Western Soc. of Naturalists, annual, Goleta, Calif. (D. Davenport, Santa Barbara College, Goleta.)

27–30. American Economic Assoc., annual, Cleveland, Ohio. (J. W. Bell, 629 Noyes St., Evanston, Ill.)

27–30. American Finance Assoc., annual, Cleveland, Ohio. (G. E. Hassett, Jr., New York Univ., 90 Trinity Place, New York 6.)

28. Society for the Advancement of Criminology, annual western, Fresno, Calif. (W. Dienstein, Fresno State College, Fresno.)

28–29. American Folk-Lore Soc., annual, Santa Monica, Calif. (MacE. Leach, Bennett Hall, Univ. of Pennsylvania, Philadelphia 4.)

28–30. American Anthropological Assoc., annual, Santa Monica, Calif. (W. S. Godfrey, Jr., Logan Museum, Beloit College, Beloit, Wis.)

28–30. American Historical Assoc., annual, St. Louis, Mo. (AHA, Study Room 274, Library of Congress, Washington 25.)

28–30. Archaeological Inst. of America, annual, Philadelphia, Pa. (C. Boulter, Library, Univ. of Cincinnati, Cincinnati 21, Ohio.)

28–30. Industrial Relations Research Assoc., Cleveland, Ohio. (E. Young, Sterling Hall, Univ. of Wisconsin, Madison 6.)

29. Mathematical Assoc. of America, 40th annual, Rochester, N.Y. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

29–30. American Chemical Soc., Div. of Industrial and Engineering Chemistry, Princeton, N.J. (A. H. Emery, ACS, 1155 16 St., NW, Washington 6, D.C.)

January

7–11. International Social Science Council, 3rd gen'l. assembly, Paris, France. (Secretary Gen'l., ISSC, 19, avenue Kleber, Paris 16.)

10. Technical and Clinical Applications of Radioisotopes, Assoc. of Vitamin Chemists, Chicago, Ill. (M. Freed, Dawe's Laboratories, Inc., 4800 S. Richmond St., Chicago 32.)

10–12. American Group Psychotherapy Assoc., 14th annual, New York, N.Y. (C. Beukenkamp, Jr., AGPA, Room 300, 345 E. 46 St., New York 17.)

14–16. Cottonseed Processing as Related to the Nutritive Value of the Meal, 4th conf., New Orleans, La. (Southern Regional Research Lab., USDA, 1100 Robert E. Lee Blvd., New Orleans 19.)

14–16. Reliability and Quality Control in Electronics, 3rd natl. symp., Washington, D.C. (C. M. Ryerson, Radio Corp. of America, Bldg. 10–6, Camden 2, N.J.)

14–18. Society of Automotive Engineers, annual, Detroit, Mich. (Meetings Div., SAE, 29 W. 39 St., New York 18.)

14–20. Indian Science Cong. Assoc., 44th meeting, Calcutta, India. (General Secretary, ISCA, 1 Park St., Calcutta 16.)

15. Society for Applied Spectroscopy, Philadelphia, Pa. (F. M. Biffen, Johns-Manville Research Center, Manville, N.J.)

16–23. Australian and New Zealand Assoc. for the Advancement of Science, 32nd meeting, Dunedin, N.Z. (J. R. A. McMillan, ANZAAS, Science House, 157 Gloucester St., Sydney, N.S.W., Australia.)

17. Constructive Medicine in Aging: Cardiovascular Disorders in the Aged, symp., Cincinnati, Ohio. (J. B. Chewning, Wm. S. Merrell Co., Cincinnati 15.)

17–18. Engineers Joint Council, New York, N.Y. (EJC, 29 W. 39 St., New York 18.)

18–19. Symposium on Blood, 6th annual, Detroit, Mich. (W. H. Seegers, Wayne State Univ. Coll. of Medicine, Detroit 7.)

21–22. Solar Furnace Design and Operation, Phoenix, Ariz. (J. I. Yellott, Assoc. for Applied Solar Energy, 3424 N. Central Ave., Phoenix.)

21–25. American Inst. of Electrical Engineers, winter general, New York, N.Y. (N. S. Hibshman, AIEE, 39 W. 39 St., New York 18.)

23–25. Very Low Frequency Electromagnetic Waves, symp., Boulder, Colo. (J. R. Wait, National Bureau of Standards, Boulder.)

25–26. Protein Metabolism, 13th annual conf., New Brunswick, N.J. (W. H. Cole, Rutgers Univ., New Brunswick, N.J.)

28–29. Many Body Problem, symp., Hoboken, N.J. (G. J. Yevick, Dept. of Physics, Stevens Inst. of Technology, Hoboken.)

28–31. American Meteorological Soc., New York, N.Y. (K. C. Spengler, AMS, 3 Joy St., Boston 8, Mass.)

(See issue of 16 November for comprehensive list)

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EXPLORATORY DEVELOPMENT

IMMEDIATE OPENING FOR Ph.D. in a biological science such as pharmacology, physiology, or biochemistry with 2 to 3 years' experience in the pharmaceutical industry or equivalent time in teaching or research in a medical school.

Excellent post in progressive, expanding pharmaceutical industry in the East. Applicant should have flair for conceiving good ideas, knowledge of screening technique and familiarity with pharmacological action of drugs.

IMMEDIATE OPENING FOR B.S. with background in biology or chemistry to serve as project administrator in coordinating scientific activities of exploratory development project team. Industrial experience highly desirable but not essential.

Company benefit program provides broad coverage. Applicants should submit full information outlining education and experience and an indication of desired salary. Address replies to:

Box 268, SCIENCE

OPPORTUNITY AVAILABLE FOR YOUNG PHYSICIAN—

in Professional Service Department of Medical Division. Should have ability and interest in medical writing. Clinical or laboratory research experience desirable. Large midwestern ethical pharmaceutical company. For further details please write and send résumé to Box 252, SCIENCE.

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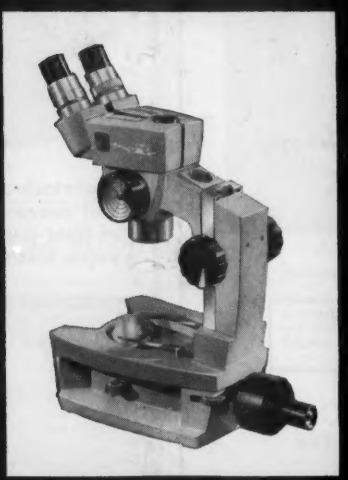

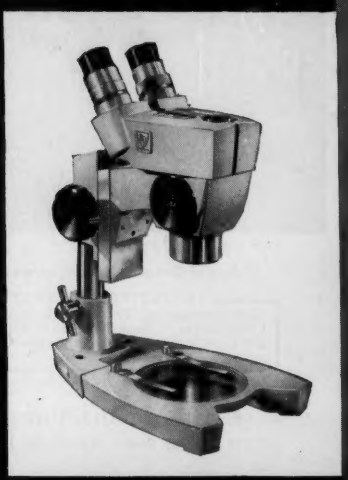

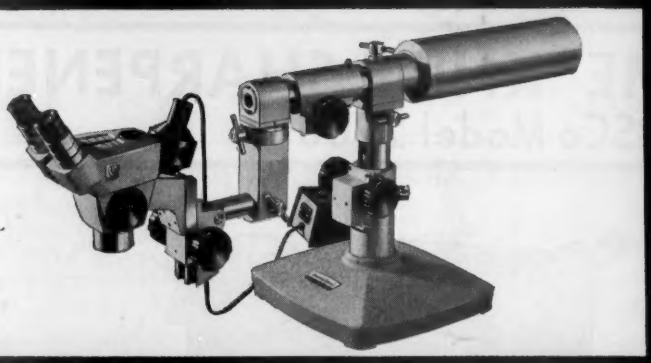

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